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ESSAYS
AND
OBSERVATIONS
ON THE
CONSTRUCTION AND GRADUATION
OF
THERMOMETERS,
AND ON THE
HEATING AND COOLING OF BODIES.

By GEORGE MARTINE, M. D.

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TO
DR. JOSEPH BLACK,
PROFESSOR OF MEDICINE
AND CHEMISTRY
IN
THE UNIVERSITY OF EDINBURGH,
THIS NEW EDITION OF
DR. MARTINE'S ESSAYS
IS DEDICATED,
BY
THE EDITOR.

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ESSAY

E S S A Y
ON THE
CONSTRUCTION AND GRADUATION
OF
THERMOMETERS.

A



Qui vel caloris vires, & calorem ipsum veluti in gradus partiri, vel materiae cui inditus est copiam quantitatemque distincte percipere, &c.—— Utinam id alii & perspicaciori praediti ingenio, & quibus in summa tranquillitate rerum naturam perscrutari licuerit assequantur; ut homines non omnium modo scientes, sed omnium fere potentes fiant.

BERNARDINUS TELESIIUS.

St. Andrews, Aug. 1738

SOME
OBSERVATIONS
AND
REFLECTIONS
CONCERNING THE
CONSTRUCTION AND GRADUATION
OF
THERMOMETERS.

WE cannot enough commend and admire that excellent invention of *Thermometers*, whereby we are enabled to make some judgment of the various degrees of heat in bodies. It is not our business at present to determine to whom we owe that noble and useful discovery; whether to Sanctorio, to Galileo, to Father Paul, or to Drebbel: for I find it ascribed to

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all these by their respective favourers and admirers*. At first, as is the fate of all other inventions, Thermometers were but rude and imperfect machines, and not easily to be applied to so many purposes as came afterwards to be in use. They were very clumsy; and, as the various degrees of heat were pointed by the different contraction or expansion of air, they came afterwards to be found uncertain, and sometimes deceiving measures of heat; as the bulk of air was affected not only by the dif-

* The invention is given to Drebbel by his countrymen Boerhaave (*Chem. I.* 152, 156.) and Musschenbroek (*Tent. Exp. Acad. Cim. Add. p. 8. Ess. de Phys. § 946*). Fulgenzio (*Life of Father Paul*, p. m. 158.) ascribes it to his master Father Paul Sarpi, that great oracle of the republic of Venice. But there was an humour prevailed, in those days, of fathering almost all the curious discoveries of the age on that great statesman and virtuoso. Vincenzo Viviani (*Vit. de. l' Galil. p. 67. See too Oper. di Galil. Pref. p. 47.*) speaks of Galileo as the inventor of Thermometers. But we know how much and how fondly he adored the memory of his great master. (See *Hist. Acad. des Scien. 1703*, p. 169, 175, 176, 180). Still all these are posthumous claims, and they too imputed by others. But Sanctorio himself (*Com. in Galen. Art. Med. p. 736, 842. Com. in Avicen. Can. Fen. I. p. 22, 78, 219.*) expressly assumes the invention in question. And Borelli (*de Mot. Animal. II. Prop. 175.*) and Malpighi (*Oper. Posth. p. 30*) ascribe it to him without reserve. And these Florentine academicians are not to be suspected of partiality in favour of one of the Batavianian school.

ference.



ference of heat, but likewise by the difference of weight of the atmosphere*.

2. † Ferdinand II. great duke of Tuscany, or the gentlemen of the *Accademia de P Cimento* under his protection, made a very great improvement on those curious and useful machines. They made them with spirits inclosed in glass tubes, and these hermetically sealed. So that they could suffer nothing by the evaporation of liquor, or the various gravity of the incumbent atmosphere. And it was Thermometers made in this way which were first introduced into England by Mr. Boyle ‡, and came immediately to be of universal use among the virtuosi in all the several countries, wherever polite learning and philosophy were cultivated.

3. But there was not so much use made of those instruments as they were capable of. We have plenty of observations made with particular Thermometers at different times and places. But then these were not constructed by any fixed scale or standard. Even the Florentine Weather-glasses themselves, whose highest term was adjusted to the great sun-shine heats of that country, were too vague and indetermined: and in other places every workman made them according to his own way and fancy, without adjusting his numbers to any

* Boyle Exp. on Cold, Abr. I. p. 577. &c.

† Vivian. Vit. de P Galil. p. 67.

‡ Exp. on Cold, Abr. I. p. 582.

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known or determined degrees of heat. And so they could not be compared one with another; nor could the observations made by different persons, and in different parts of the world, be collated with any degree of certainty or justness. By which means, notwithstanding all the very numerous registers of the weather that have been kept and published by different authors, we are still at a loss to determine the comparative differences of heat and cold in different countries and climates, and the result of many other observations.

4. Had all the Weather-glasses in the world been made according to one determined scale, these inconveniencies and uncertainties would have been prevented; which are now unavoidable, and must still continue so, until every body agree to graduate their Thermometers in the same way, or at least determine some fixed or unalterable points of heat, to which all the different scales of those instruments may be reduced.

5. The great and truly honourable Mr. Boyle* found himself very much at a loss for a standard, whereby to measure heat and cold: the common instruments shewing him no more than the relative coldness or heat of things, but leaving him in the dark as to their positive degrees. Whence he could not communicate the idea of any such degree to

* Exp. on Cold. Abr. I. p. 579.

another person. Thermometers were then such indefinite and variable things, that it seemed morally impossible from them to settle such a measure of heat and cold, as we have of time, distance, weight, &c. there having been then no method thought upon to compare together any two different Thermometers; or the observations made by them. Now as bodies are variously affected by various heats, regularly undergoing such and such changes, at such and such degrees of it, so the fairest way to fix a standard method of adjusting Thermometers would seem to be from some remarkable change a body underwent by the application of a certain degree of heat to it. In consequence of which Mr. Boyle proposed the freezing of the essential oil of aniseeds as a term of heat and cold that might be of use in making and judging of Thermometers; and so to graduate them from this point according to the proportional dilatations or contractions of the included spirits. He mentioned too the coldness requisite to begin the congelation of distilled water as another fixed term that might be proposed: for he was persuaded that among the ordinary waters some were apt to freeze more easily than others. But the objections which he apprehended might be made to this method scared him so much, that he prosecuted no farther this consideration of fixing a standard for making and graduating Thermometers all in the same way. Which 'tis pity one of his genius and industry, and assisted with such opportunities, did not carry farther; as it is

of

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of such importance on a thousand occasions in the history and philosophy of Nature.

6. The ingenious and acute Dr. Halley * was likewise very sensible of the bad effects of that indefinite way of constructing Thermometers; and wished to have them adjusted to some determined points. He, with Mr. Boyle, lays aside the freezing of liquors, as being, in his opinion, points of heat not so justly determinable but with a considerable latitude. And what he shews the greatest fondness for is the degree of *temperature*; such as in places deep under ground, where the heat in the summer, or cold in winter have been found to have no manner of influence. Thus in a cave cut straight into the bottom of a clift fronting the sea to the depth of 130 feet with 80 feet of earth above it, Mr. Boyle † found the spirit in the Thermometer to be raised to the same height both in summer and winter. And Messrs. Mariotte, De la Hire, and Maraldi assure us, that in the cave under the royal observatory at Paris the heat continues always the same, scarcely altered by the most sultry summer heats, or bitterest colds of that country.

But, with Dr. Halley's leave, this degree of *temperature* I do not think a very convenient term for an universal construction of Thermometers. Every body cannot go to Mr. Boyle's grotto: and it is

* Phil. Trans. Abr. II. p. 36.

† Mem. Hist. of the Air. Abr. III. p. 54.

but:

but few who can have an opportunity of making observations, and adjusting Thermometers in the cave of the Parisian observatory. And we are not sure that other deep pits would coincide precisely with the temperature of these places. The difference of soils and different depths might occasion a considerable variation.

7. Another term of heat Dr. Halley * thought might be of use in a general graduation of Thermometers is that of *boiling spirit of wine*. "Only
" it must be observed, that the spirit of wine used
" to this purpose be highly rectified; for otherwise
" the differing goodness of the spirit will occasion
" it to boil sooner or latter, and thereby pervert
" the designed exactness." He objects too its great aptness to evaporate, and that in length of time it becomes, as it were, effete, and loses gradually a part of its expansive power. But truly this last mentioned difficulty, though repeated after Dr. Halley by several others, and commonly pretty much insisted on, would seem to be of no great weight. Well rectified spirit of wine, if sealed up in a glass, is pretty unalterable. It cannot evaporate: and by many years experience its force of expansion has continued the same; as, beside other observations, we know especially from the annual registers of Mr. De la Hire's spirit Thermometer,

* Phil. Transf. Abr. II. p. 35.

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that has been kept in the observatory above these threescore years by-past.

8. But a much more convenient term of heat, though less insisted on by Dr. Halley, is that of *boiling water*. This he found * to be a very fixed and settled degree of heat; and which, when once water has acquired it, cannot be increased by any longer continuance or length of time. And this point of heat Sir Isaac Newton and Mr. Amontons were justly very fond of in settling the limits of their respective Thermometers: wherein too they have been imitated by all those that came after them.

9. Though I could scarcely find any sensible difference in various waters, differing considerably in weight, saltness, cleanness, &c. so as that Mr. Taglini's objections † against the fixedness of this point are of no force; yet truly this heat of *boiling water* is not at all times and places so absolutely fixed and invariable, as most people are ready to conclude from Dr. Halley's and Mr. Amontons's observations. The fixedness or volatility of water depends very much not only on the cohesion of its particles, but also on the pressure of the incumbent

* Phil. Transf. Abr. II. p. 34. See too Amontons in Mem. Acad. Sc. 1699. p. 156. 1702. p. 210.

† See Mem. Acad. Sc. 1730. p. 714.
atmosphere.

atmosphere: * for, in a vessel much exhausted of air, water and other liquors, being freed of most of the ordinary pressure they undergo, boil in a very moderate degree of warmth, much below what in the open air is usually requisite to set them a boiling. And, agreeable hereto, Fahrenheit actually found†, that water was capable of a greater or less degree of heat in boiling, according to the greater or less weight of the atmosphere, or the greater or less height of the Mercury in the Barometer. But in ordinary changes of the weather the difference is not very great‡. And farther, we may avoid all errors that might arise from any thing of that sort, if we make our observations on the heat of boiling water, and adjust this term of heat at a middle

* See Boyle *Physico. Mech. Exp. Abr.* II. p. 473, 474. † Newton. *Opt.* p. 318.

‡ *Phil. Trans. Abr.* VI. 2. p. 18. Boerh. *Chem.* I. p. 171.

‡ It is not so great as Dr. Boerhaave, (*Chem.* I. p. 171.) in giving an account of Fahrenheit's observations, reckoned. In an alteration of 3 inches in the height of the Mercury in the barometer, he says, the boiling heat is found to differ 8 or 9 degrees. I did not find it to vary so much by the different weights of the atmosphere. From the experiments I have made I judge that, by the rise or fall of the quicksilver an inch in the barometer, the boiling heat of water varieth somewhat less than 2 degrees.

state

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state of the atmosphere in places near the level of the sea, when the mercury in the barometer stands at about 30 inches or a very little under it. And the same caution will be necessary in judging of the heat of *boiling spirit of wine*, or of the *boiling heat* of any other liquor.

10. It was on this principle chiefly of the determined fixedness of the heat of *boiling water*, that Mr. Amontons* contrived his universal Thermometer. It was indeed a pretty contrivance: but it never was, nor never could be, of universal use; nor could it answer the design of its ingenious author. Its construction is too difficult and complex; the machine itself is too bulky and incumbered to be easily managed, or carried from place to place. And then it is supposed to be fitted at a due temperature of the air, as in the spring and autumn at Paris: which, beside that it is in itself something too indefinite, at other times and places we cannot well be sure of. And the air of that temperature he reckoned to be dilated just one third by the heat of boiling water. But the dilatation of the air is not so regularly proportional to its heat; nor is its dilatation by a given heat near so uniform as he all along supposed. This depends much on its moisture†: for dry air does not expand near so

* Mem. Acad. Sc. 1702, p. 204, &c.

† See Hist. Acad. Sc. 1708, p. 15. Mem. p. 370. Muffchenbr. Tent. Exp. Acad. Cim. Add. p. 40. Eff. de Phys. § 1402.

much by a given heat, as air stored with watry particles; which by being turned into steam increase vastly the seeming volume of the air. So then as Mr. Amontons's Thermometer is affected by all these and other inconveniences*, it is no wonder it was imitated by very few, and never came to be of general use in the world.

11. From this one determined point of the heat of *boiling water* there may, however, be laid down a general method of constructing Thermometers all in the same way, so as that they shall correspond with one another in all the various degrees of heat. It is, as Mr. Boyle† proposed long ago, by marking the degree of expansion or contraction of the fluid of the Thermometers, as the heat applied is either greater or less than that of *boiling water*. Supposing, for example, the whole volume of this fluid to consist of 10,000 parts, it is to mark on the tube where that volume is expanded by heat, or contracted by cold, 1, 2, 3, 4, &c. of these parts. All which may be done at any time by different persons and in different places, so as that they shall answer precisely to one another.

12. This is a very plausible method, and has been actually tried by some, as we shall afterwards have

* See Mr. de Reaumur in Mem. Acad. Sc. 1730. p. 654.

† Exp. on Cold. Abr. I. p. 579.

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occasion to see. And yet in practice it will not be found very easy to determine exactly all the divisions from the alteration of the bulk of the fluid; beside other unavoidable inconveniences and uncertainties we shall by and by have occasion to take notice of. It is sufficient, and will be much easier and more convenient, in settling an universal method of constructing Thermometers, to determine at least two fixed points of heat at a considerable distance the one from the other; and to divide on the tube or scale the intermediate space into any convenient number of equal parts or degrees. *Boiling water*, in the circumstances we proposed, we just now found to be a pretty uniform and fixed degree of heat. And *water just freezing*, if that come out always the same, will answer the purpose most readily of any degree we could think of.

13. It is true, Dr. Halley * thought this to be a point admitting a considerable latitude. And some have suspected that water freezes at different degrees of heat in different seasons, countries and climates. And Dr. Cyrilli's observations † would seem to confirm it. At Naples he found water to freeze when his Thermometer was 10 degrees above the freezing point as it had been constructed in

* Phil. Trans. Abr. II. p. 36.

† Phil. Trans. No. 424, p. 336. No. 430, p. 189, No. 434, p. 407, 408. No. 435, p. 464.

England by the directions of the Royal Society. And Father Martini * talks of the frosts in the province of Pekin in China as greater than its latitude of 42 degrees would induce one to expect, the rivers being often frozen for four months together, &c. And adds, that it is surprising the Europeans should remain unaffected by this cold; and slight it as unable to produce ice in their quarter of the world. From all which one might be ready to conclude, with Dr. Derham†, Prof. Musschenbroek‡, and others, that the farther north we go, it takes the greater degree of cold to freeze water. And what might strengthen such a suspicion is, that I found some of the Dutch mercurial Thermometers made at Amsterdam in which the *freezing point* was marked at *gr. 32*, to be a degree or two lower here when immersed in thawing snow or beaten ice beginning to melt.

14. But I am convinced all this seeming difference flows from the carelessness of observers, or errors of the workmen. Or what if Dr. Cyrilli kept his weather-glass shut up and sheltered in a house while it froze abroad? As I know is but too ordinary with those who pretend to keep registers of the weather. To satisfy myself in this article, I took a very

* Atlas Sinenf. p. 27. See too Boyle Ex. on Cold. Abr. I. p. 577. Reaumur in Mem. Ac. Sc. 1734, p. 233.

† Phil. Transf. ibid.

‡ Eff. de Phys. § 913.

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fair method of inquiring if there was really any such difference in the cold of *freezing water* in different climates. I marked two mercurial Thermometers here in the latitude of 56 deg. 20 min. and got a correspondent of mine to mark some others at London in the latitude of 51 deg. 32 min. all at the freezing point in snow or powdered ice melting: and, upon exchanging these Thermometers, we found them, when again immersed in thawing snow or ice, still to point at the very same marks precisely without the least observable difference. Were there any odds occasioned by the difference of climates, would not near 5 degrees of latitude have shewn it very sensibly *? And at this same very mark does the quicksilver stand in such thawing ice or snow at all seasons of the year, in summer or winter, in cold weather or warm weather, under a light or a heavy atmosphere, &c. From whence I conclude the *freezing point* to be a very constant and settled degree of heat, more fixed and determined than even that of *boiling water*, and

* The universality and fixedness of the term of congelation I can now affirm with still greater confidence and certainty after the trials I lately made, not only at London, but at Paris in the latitude 48 deg. 50 min. and Dijon in the latitude 47 deg. 20 min. by which I found, that precisely the same degree of cold was requisite to freeze water in Burgundy and in the isle of France, as in our northern climates.

consequently

consequently very fit to be one of the fixed limits in adjusting our Thermometers.

15. The fixedness then of the *freezing point* I think we may take for granted, and sufficiently established by these observations, notwithstanding what Professor Taglini may have said to the contrary; and the unnecessary concessions Mr. De Reaumur thought fit to make on that head*; and the observations adduced by Dr. Musschenbroek†, by which he would prove the *freezing of water* to begin in various degrees of cold, depending on some saline additional mixture from the air. All which may be allowed to shew, that in some places and circumstances water freezes more or less easily, though the degree of cold in this act of *natural freezing* be, for any thing we can perceive, in all pretty nearly the same.

16. The cold of *water beginning to freeze*, or, which is the same thing, of *ice beginning to melt*, is such a convenient fixed point, such a remarkable period of heat and cold, and withal so easily determined by emerging the bulb of the Thermometer in thawing snow or ice, that it is surprising so few have taken it into the scale of their Thermometers, that theirs might be the better compared

* Mem. Acad. Sc. 1730. p. 712.

† Tent. Acad. Cim. Add. p. 184, 185. Ess. de Phys. § 913, &c.

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with other peoples observations. We are not absolutely sure even of this part of the scale of the Florentin weather-glasses. And the French have not yet precisely determined the *freezing point* in Mr. De la Hire's standard Thermometer of the Observatory, by which the register of the weather has been so very long kept; and with which they sometimes compare other observations: and most of the other observations of the weather, both in the Memoirs of the Academy of Sciences and in the Transactions of the Royal Society, are done with Thermometers so negligently constructed, that we are left altogether uncertain of their degrees of heat: most of those that have the *freezing point* marked on them having it very erroneously graduated, and also having no other determined or known period of heat; and so coming to be as indefinite and uncertain as the others.

17. The great Sir Isaac Newton thought the settling the degrees of heat and cold well worth his notice; and, as he carried every thing he meddled with beyond what any body had done before him, and generally with a greater than ordinary exactness and precision, so he * laid down a method of adjusting Thermometers in a more definite way than had been done hitherto. We have already † found

* Phil. Trans. Abr. IV. 2. p. 1, &c.

† § 12.

it necessary, at least convenient, to fix on two determined periods of heat to make up an intelligible scale of its various degrees. We have likewise found *water freezing* and *water boiling* to be in two points of heat very convenient for such a purpose. And these are the very degrees of heat he pitched upon. The liquor he used was Lintseed Oil, a homogeneous fluid enough, and capable of a considerable rarefaction, and not having been observed to freeze even in very great colds, and able to bear a great heat without boiling. Supposing the bulb when immersed in *thawing snow* to contain 10,000 parts, he found the oil expanded by the heat of the *human body* so as to take up $\frac{1}{32}$ more space, or 10,256 such parts; and by the heat of *water boiling* strongly 10,725; and by the heat of *melting tin* 11,516, beyond which he did not carry his oil Thermometer. So that, reckoning the *freezing point* as a common limit between heat and cold, he began his scale there, marking it *gr. 0*; and the heat of the *human body* he made *gr. 12*; and consequently the heat of *boiling water* was expressed by *gr. 34* = $\frac{725 \times 12}{256}$; and *melting tin* by *gr. 71*.

18. I wish the world would have received this or any other determined scale for adjusting their Thermometers. But I suppose they might be apprehen-

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five of some inconveniences in this scheme. Beside some inconsiderable arithmetical errors in Sir Isaac's paper, and some few not accurately enough made observations of no great moment, they would allege *, that he had expressed himself as if he had thought the point of *freezing water* to be the lowest degree of heat. And indeed he has not mentioned any winter cold beyond that; though, as we shall take notice on another occasion, we sometimes experience vastly intenser colds: in consequence of which it would be easy to protract Sir Isaac's scale downward below *gr. 0*, or the freezing point, and so to make it as serviceable in common use, even for great degrees of cold, as other Thermometers.

But there is another difficulty which will hold in all oil Thermometers, or any made with a viscid liquor, that it adheres too much to the sides of the tube. In a sudden cold or fall of the oil, a good deal sticks by the way, and only sinks gradually after, so that at first the surface appears really lower than the present temperature requires. And beside, as at all times some must continue to stick and moisten the inside of the tube, in different degrees of heat and cold, the oil becoming alternately more or less viscid, will adhere sometimes more and sometimes less; and therefore will inevitably disturb the regularity and uniformity of the Thermometer.

* See Amontons in Mem. Acad. Sc. 1703. p. 233.

19. The Florentin Thermometers made with spirit of wine were by no means so liable to errors of this kind. The spirit indeed must moisten the inside of the tube: but as this is so very thin a liquor, it is probable that moistening is always pretty uniform, and extremely thin; so as that it can have no other effect but to render the tube a very little narrower. And so truly rectified spirit of wine has been more used in Thermometers than any other liquor. It is very ticklish, is easily and very much affected by heat and cold; nor does it soon gather air bubbles, as watry fluids are ready to do even in no very great heats. But, as we said before *, those spirit Thermometers did not use to be reduced to a fixed standard. At length Mr. De Reaumur† has in a very ingenious way attempted to establish a general construction of such Thermometers, which might be copied at all times, and in all countries: and so to settle, as it were, a general correspondence of observations that should be made by such instruments. He adjusted his Thermometer to the *freezing point* by an artificial mixture: and then, immersing it in *boiling water*, found how much it was dilated by that heat, in the way Sir Isaac Newton had managed his oil Thermometers. The quantity of spirits in his glass he supposed to be 1000 parts; and he found to the bulk of how

* § 3, 4, 16.

† Mem. Acad. Sc. 1730. p. 645, &c.

many

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many of these parts the liquor was dilated by that boiling heat. The stronger the spirits, this dilatation is found always the greater. The best spirit of wine commonly sold, from this *artificial freezing of water* to that *boiling heat*, was dilated $87\frac{1}{2}$ of those parts *: and a mixture of such a strong spirit and water in equal proportions (which would be about the state of common brandy) was expanded only $62\frac{1}{2}$ such parts †. He speaks too of a spirit so strong as to have the dilatation of 90 in its *boiling heat* ‡. But what he judged most convenient for his ordinary Thermometers was a spirit of such a degree of strength as in the above period of heat to be dilated just 80 of these 1000 parts **.

20. This intimation of Sir Isaac Newton with spirit of wine instead of oil is a promising enough method of making a settled standard scale for Thermometers; and accordingly one of them constructed by Mr. De Reaumur himself is now placed in the Royal Observatory at Paris; and a journal kept of the weather by it. And others constructed in the same way have been sent to different quarters of the world to compare the degrees of heat of different climates: a project long ago of the great Colbert's †, but never till now satisfactorily

* Mem. Acad. Sc. 1730 p. 690. † Ibid. p. 692.

‡ Ibid. 1734. p. 261.

** Ibid. 1730. p. 697. 698. and fig. 8.

† See Mem. Acad. Sc. 1702. p. 209.

executed. Observations have been made with them in the Torrid Zone. And the French Academicians had them in their philosophical embassy to the north for determining the figure of the earth. And yet I am afraid these Thermometers are constructed on principles, that will by no means be found so accurate as were to be wished and expected in such a case. Mr. De Reaumur* determines his freezing point, not from *thawing snow* or *ice*, but from an *artificial congelation of water* in warm weather. And as he† uses very large bulbs for his glasses, it may be 3 or 4 inches in diameter, I am apt to think that, before the due temperature of the surrounding ice can be thoroughly propagated to the whole contained spirits, it will be quite melted down again; and so the *freezing point* marked much higher than what it should be‡. He indeed †† speaks of a quarter of an hour as sufficient to bring the contained spirit to the temperature of the ambient *medium*. But as far as I can judge from my observations (and many I have made

* Mem. Acad. Sc: 1730. p. 655. 656. 680. 681. 712.

† Ibid. p. 660. 710.

‡ From collating some observations together I reckon Mr. De Reaumur's mark of freezing water, instead of coinciding with Fahrenheit's *gr.* 32, as it should do, to correspond with *gr.* 34. or a little above it.

†† Ibid. p. 711.

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on this subject) it would rather take some hours to penetrate thoroughly such a great bulk of any liquor, and reduce it fully to its own degree of heat. And so Thermometers of great bulbs can never be used with success, or trusted to in common experiments of the heat of bodies, or observations of the weather: as we may be satisfied by collating the several observations in the Royal Observatory made with De la Hire's and De Reaumur's Thermometers, which at different times I find to quadrature very ill together, just I suppose from that cause of the different sizes of their bulbs *. For large bulbs can never be so quickly influenced by great and sudden variations of heat and cold, as to make these changes, if they last but a short while, sufficiently remarkable, or to shew their real and full quantities. Small bulbs and small tubes are (notwithstanding the imaginary faults and difficulties started against them by Mr. De Reaumur †) vastly more convenient, and may be constructed sufficiently accurate. I have them made with capillary bores that correspond almost precisely to one another in all degrees of heat; and consequently must be allowed to have all the exactness that can be imagined or desired in such instruments.

* The diameter of the bulb of Mr. De Reaumur's I found to be about $3\frac{2}{3}$ inches; while the other was less than 2 inches.

† Mem. Acad. Sc. 1730. p. 650. 656. 659.

21. But

21. But if we cannot trust to Mr. De Reaumur's *point of congelation*, what shall we say to his other fixed term, to wit, the heat of *boiling water*? This, I humbly think, is, under his management, still more vague and uncertain. As water, were it surrounded by melted tin, though it would boil and foam and suffer very great agitation, is yet incapable of being warmed beyond its ordinary great boiling heat, and continues always much colder than melted tin; just so, as I experienced upon trial, spirit of wine, though immersed in boiling water, can never acquire near such a great heat, but keeps always a good deal under it, though bubbling and foaming and tossed to a very great degree. The spirit then in the Thermometer is absolutely incapable of such a great heat as Mr. De Reaumur ascribed to it; and that not by a small or trifling difference. I find highly rectified spirit of wine cannot be heated beyond *gr. 175* or so in Fahrenheit's Thermometer, while boiling water raiseth the quicksilver 37 degrees higher: and common brandy was able to conceive a heat no greater than about *gr. 190*. So far was Mr. De Reaumur in the wrong when he thought that all spirits, weak and strong, immersed in *boiling water* received a given degree of heat and that equal to the heat of the surrounding water. I suppose his standard spirit could take on a heat of about *gr. 180*; less by 32 degrees than what he reckoned.

C

22. Beside

22. Beside all this, I truly think spirit of wine, though good enough for ordinary weather-glasses in temperate climates, is yet not so fit a fluid whereof to make standard Thermometers. It is incapable of bearing very great heats, or very great colds. It boils sooner than any other liquor; and though it keeps fluid in pretty strong colds, yet it would seem, from some experiments, that it does not condense very regularly in them: and at Torneao near the polar circle the winter cold was so violent, that the spirits were frozen in all their Thermometers *. So that the latitude of heat spirit of wine is capable of expressing is by much too limited to be of very great or universal use.

23. What fluid then shall we take for our Thermometers? We have found inconveniences in air, oil, and spirits; and water is more exceptionable than any of them. We have, it seems, nothing left but quicksilver. This is a very moveable and ticklish fluid; it both heats and cools faster than any liquor we know, or have had occasion to try; faster I am sure than water, oil, or even spirit of wine; it never freezes by any degree of cold hitherto observed, and bears a great deal of heat before it arrives at a boiling expansion, and, if well purified, does not wet or stick to the inside of

* See de Maupert. Fig. de la Terre, p. 58.

the tube. Dr. Halley *, though apprised but of some of these remarkable properties, thought it a fluid very fit for Thermometers, were but its expansion more considerable. However, as any inconvenience arising from this consideration may be avoided by making the bulb have a great proportion to the tube, so it is now very deservedly come to be in the greatest use in the Thermometers that people would most trust to. It is said † that they were first contrived by that curious mathematician Olaus Roemer. Mr. Fahrenheit in Amsterdam, and other workmen in that country, manufactured very many of them, and that in a portable and mighty convenient form for many purposes, making them very small, and inclosing the tube in another glass hermetically sealed. From Holland they were distributed into different quarters of the world, and have been imitated in some other places. And now they are made no where in greater perfection, or with greater exactness, than by our countryman Wilson at London.

24. As Roemer's or Fahrenheit's scale is convenient enough, I wish it were universally kept and used, to preserve an uniformity in our observations; and so that every body, when they speak of experiments wherein the heat of bodies was adjusted,

* Phil. Trans. Abr. II. p. 34.

† See Boerh. Chem. I. p. 72c.

28 The CONSTRUCTION and GRADUATION

or pretended to be observed by Thermometers, were to have the same language; and so be universally understood.

In this Thermometer the bulb is supposed to contain, as Dr. Boerhaave * and Dr. Musschenbroek † tell us, 11,124 parts of quicksilver, which stands at the lowest mark, or *gr. 0*, in a very intense intolerable cold, being surrounded with a mixture of snow or beaten ice and sal ammoniac or sea-salt ‡.

If the same bulb be immersed in *snow* or *ice thawing* naturally, or in *water beginning to freeze*, the quicksilver is dilated, and so rises in the tube 32 of these 11,124 parts; and therefore the space of the tube from *gr. 0* to the *freezing point gr. 32* is divided into these 32 equal parts ||.

When the Thermometer is placed in water brought to strong *boiling*, (the atmosphere being in the condition formerly mentioned **), the quicksilver is dilated 212 of these parts beyond its original bulk of 11,124, so as now to possess in the bulb and tube together a space equal to 11,336 such parts: and the space from *gr. 32* to *gr. 212* is divided into 180 equal parts or degrees of the Thermometer ††; which, if the tube be long enough,

* Chem. I. p. 174. † Eff. de Phys. § 948.

‡ Fahrenh. in Phil. Transf. Abr. VI. 2. p. 52. Musschenbr. Diff. Phys. p. 680. Eff. de Phys. § 948.

|| Fahrenh. in Phil. Transf. Abr. VI. 2. p. 52.

** § 9.

†† Fahrenh. in Phil. Transf. Abr. VI. 2. p. 18, 52.

may be protracted as far as is convenient. It may go well enough to *gr.* 600, and not much farther; for, with a heat but little greater than that, the Mercury begins to boil.

25. Dr. Boerhave is not always quite consistent with himself in his accounts of the number of parts into which the Mercury in the bulb is supposed to be divided. In another place *, instead of 11,124 he reckons only 10,782, and afterwards † he raises it to 11,520, (which I take to be nearer the truth), and yet still speaks of a given heat as expanding the quicksilver the same number of parts as in the other supputation. Which of his suppositions is right I am not absolutely sure of before making trial, nor need we much to care: for, without all that *apparatus*, I humbly think the easiest and surest way too, and what in practice I have followed, is not to be solicitous about the bulk of the quicksilver, but to fill the bulb and tube so, as that in freezing water, or melting ice, the Mercury shall stand at a convenient height, which must be very nicely marked *gr.* 32, and then as accurately to observe where it stands when dilated by the heat of boiling water to *gr.* 212. The intermediate space is then divided into 180 degrees; which scale may be protracted upwards and downwards as far as we shall judge convenient. In the construction of

* Chem. I. p. 165.

† Ibid. Expl. Tab. V. fig. 3.

30 The CONSTRUCTION and GRADUATION

these Thermometers the two great limiting or fixed points being *freezing water* and *boiling water*; which we found to be the most convenient periods of heat for adjusting these instruments.

26. Indeed in all this we have supposed the bore of the tube to be perfectly cylindrical; which cannot always be obtained. But though it be tapering or somewhat unequal, it is easy to manage that matter by making a small portion of the quicksilver, as much for example as fills up a half, or, if you please, a whole inch, slide backward and forward in the tube; and by this means to find the proportions of all its inequalities, and from thence to adjust your divisions to a scale of the most perfect equality.

27. The conceiving the bulk of the contained mercury at the beginning of the scale to be either 10,782, or 11,124, or 11,520, or, as I guess it should be, near 11,790 parts, and its expansion from thence by the heat of boiling water to be 212 of these parts, was, to be sure, a division originally quite arbitrary. And I confess there might have been a more convenient one fixed upon at first, and adopted yet, if this were not now so universally known and used; and Thermometers constructed in that form every where scattered up and down, and in the hands of most of the Virtuosi in Europe. I shall readily allow that Mr. de l'Isle's at
Peterburgh

Petersburgh might have had a greater look of simplicity. * In his the whole bulk of quicksilver, when immersed in *boiling water*, is conceived to be divided into 10,000, or rather 100,000 parts: and from this one fixed point the various degrees of heat either above or below it are marked in these parts on the tube or scale, by the various expansion or contraction of the quicksilver in all the imaginable variety of heat. "As pure mercury," says he, "is of the same nature every where, nor is liable to any alteration from being inclosed in a tube; and as it is probable that, taking it equally purified, it will in different countries be subject to the same expansion, if exposed to the same degree of heat; for this reason he is persuaded these Thermometers may very well serve to compare the temperature of different countries." And indeed such a construction bids fair for being a very good standard method for graduating Thermometers all in the same way; and is much less liable to uncertainties than that similar one of Mr. De Reaumur, whereof we have already given an account †. However, if Mr. De l'Isle had pleased, I cannot but think it would have been rather something better to have made the integer of 100,000 parts, or fixed point, at *freezing water*; and from thence to compute the dilatations or con-

* Phil. Transf. N. 441. p. 222.

† §. 19, 20, 21.

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denfations of the quicksilver in those parts. All the common observations of the weather, &c. would have been expressed by numbers increasing as the heat increased; which surely is the more natural way. Nor would there have been great incongruity or inconvenience in expressing, after the manner of Mr. De Reaumur, the few observations we have below simple freezing by numbers of contraction below *gr. 0*, or 100,000.

28. But, as I said before †, however promising in theory such a scheme may appear, it will not in practice be found very easy to determine exactly all the divisions from the alteration of the bulk of the contained fluid. And beside, as glass itself is dilated by heat, though in a less proportion than quicksilver, so that it is only the excess of the dilatation of the contained fluid above that of the glass that is observable; if different kinds of glass be differently affected by a given degree of heat, this will make a seeming difference in the dilatations of the quicksilver in the Thermometers constructed in the Newtonian method, either by Mr. De Reaumur's, or Mr. De l'Isle's rules. Now Dr. Boerhaave *, from Fahrenheit's observations, speaks of different kinds of glass as very differently affected by heat. And upon enquiry I was informed that Dr. Musschenbroek found by his Pyrometer, that

† § 12.

* Chem. I. p. 141.

tubes of different sorts of glass were variously affected, some more, some less, by the same degree of heat. From whence Thermometers constructed of such different sorts of glass would necessarily make the seeming dilatation of the contained fluid appear at different degrees in the very same point of heat. And accordingly Mr. Campbell, an ingenious and worthy member of the Royal Society, has, by experiments made with all imaginable exactness and scrupulosity, found, in Thermometers constructed in Mr. De l'Isle's way, the quicksilver to stand at different degrees of the scale when immersed in thawing snow. In some it was at *gr.* 154, in others at 156, and in another at 158: and emptying the tubes, and preparing them again, and that sometimes with other mercury, he always, even in a great many trials, found the result come out the very same. So careful and exact had he been that the same tubes give always the same degrees of dilatation as nearly as could be expected in such nice and subtle experiments. One of Mr. De l'Isle's own Thermometers, which I tried very carefully, gave me always the quicksilver at about *gr.* 150. In another of his, where on the one side he has put Fahrenheit's scale, I see *gr.* 32 over against his own *gr.* 148 $\frac{1}{4}$. In one constructed by Mr. De Monier at Paris on De l'Isle's principles I saw the *terme de congelation* put down at *gr.* 148. Nay by Mr. De l'Isles own accounts I find his weather-glasses disagreeing considerably from one another.

34 The CONSTRUCTION and GRADUATION, &c.

another. So uncertain and precarious would this way of constructing Thermometers seem to be. From all which I humbly think, that it is better and more convenient to drop this method altogether, and to fix on two points of heat at a considerable distance the one from the other; and to divide the intermediate space of the tube into a convenient number of equal parts or degrees: as we just now proposed * to be done in our imitations of Roemer's or Fahrenheit's Thermometers.

29. To enlarge our views of heat, and to enable us to compare other Thermometers with these, (which after this I wish were always to be the standard), it would not be amiss to observe a great many remarkable periods of heat and cold that have been taken notice of by others, and to determine where they fall on Fahrenheit's scale. We should determine about what degrees on it the several sorts of weather are marked; the various degrees of animal heat; what degrees of heat reduce such and such consistent bodies into a fluid state; the quantities of heat required to boil the fluids we have occasion to try, &c. But the comparing the different Thermometers which have been in use in the world, and the determination of the various degrees of heat in bodies make fruitful enough subjects for other curious and useful enquiries.

* §. 24, 25, 26.

THE
COMPARISON
OF
DIFFERENT THERMOMETERS.

LONDON, 1740.

A N
E S S A Y

TOWARDS COMPARING
DIFFERENT THERMOMETERS
WITH ONE ANOTHER.

WE had occasion formerly to take notice of the great uncertainty of thermometrical observations by reason of the vague and inconstant way that people had of making their instruments. However, it will be worth while narrowly to enquire, as far as our lights can carry us, into the principles on which they were constructed; and if we can find out these principles so as to compare the old Thermometers with any regular one we are well acquainted with, we shall recover, as it were, the lost observations of our predecessors; lost for want of knowing the meaning of their numbers and graduations. And then observations made at different times and places by any known instruments

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ments will be no longer useless memorandums. We shall be able to compare them with one another, and with our own observations, and with the degrees of heat we are acquainted with.

2. As Sanctorio seems to be the first who had a notion of measuring the various degrees of heat by the dilatation of bodies, people will be apt to expect, and many will fondly wish, that he had left us his method of numbering the degrees of his Thermometers. Though, to acknowledge the truth, it would be a matter of more curiosity than use, as they were affected by the difference of weight of the atmosphere, as well as by the difference of heat of the ambient medium. To remedy which inconvenience the virtuosi of the Cimentin Academy contrived their Thermometers in a better way; carefully sealing up the included spirits from any influence of the gravity of the atmosphere; and marking on the tubes, or on the scales fixed to the tubes, numbers whereby to judge of the various dilatations of those spirits by the various degrees of the heat applied.

I am surprised to find Dr. Musschenbroek *, who had been at the pains to give a Latin translation of their whole work, and to enlarge and embellish it with excellent additions, representing these Florentine Thermometers as constructed in a very loose,

* *Ess. de Phys.* § 247. p. 5.

arbitrary and indefinite way, and without any rule at all. This is an imputation to which they are less liable than many others that came after them. They made them of several sorts, some with greater and some with less accuracy. But in the constructing of those in which they could most confide *, they tell us expressly at what degree the spirit stood in the ordinary cold of simple ice or snow. Which, as their experiments were done within doors in Italy, I take commonly to have been in a thawing state, and consequently to point out to us the degree of freezing water, which we found † to be a very fixed period of heat and cold, coinciding we know ‡, with our *gr.* 32. Thus in their 1st sort this point of heat fell at *gr.* 20 §; and in their 2d to about *gr.* 13½ ||.

So then we have one fixed and intelligible point of heat in their Thermometers, which is more than we have in those of many others. But from one point we cannot judge of the rest of the numbers: and it must be confessed their highest degree of heat is not defined in a way we could have wished. They tell us that in their first or long Thermometer the spirit, when exposed to the great midsummer heats in their country, rose to where they

* Tent. Exp. Acad. Cim. p. 5.

† Eff. I. § 14.

‡ Ibid. § 24.

§ Tent. Exp. Acad. Cim. p. 2, 7.

|| Ibid. II. p. 129.

marked *gr.* 80 *. And the spirit in the 2d or smaller Thermometer in such a heat stood at *gr.* 40 †. This indeed is a very incongruous way of graduating Thermometers; as the great heat of the summer sun is such an indefinite degree of heat in different days, years, climates, &c. And yet, by good chance, there is left us a way to judge of the particular summer heat they happened to pitch upon; and of finding whereabouts it would have fallen in a Thermometer constructed in Fahrenheit's way, which we have taken up as our standard. To affirm this will, I know, be looked on at first sight as pretending to a thing in itself lost and now impracticable. But Borelli and Malpighi, two curious and careful observers of nature, luckily had occasion to tell us, that the *vicera* of some hot animals, such as of cows, deer, &c. raised the spirit in the small Florentine Thermometer to about *gr.* 40. the very degree of this their summer sun heat. And that animal heat I find to coincide with *gr.* 102. or thereabouts in ours. From which two periods of heat determined in both Thermometers we can make a pretty good estimate of all observations made by any of the regular and well constructed Florentine glasses; as we may see by looking on the table at No I. II. III.

3. The Thermometers that came to be used in the world were all imitations of those of the *Aca-*

* Tent. Exp. Acad. Cim. p. 2, 4. † Ibid. p. 4.
demia

temia de P Cimento, but not constructed in such a regular way or upon such determined principles. In France Mr. Hubin was employed in making them for the *Academie des Sciences* at Paris. But we find they were not made all perfectly alike. The Indian missionaries * tell us, that they used those that were graduated lowest. However, we may reckon at a medium that which Mr. Amontons gives us a view of in the *Memoirs* †, under the name of the *Ancienne Thermometre*. In it the freezing point was at *gr.* 25, corresponding consequently to Fahrenheit's *gr.* 32; and the heat of the cave of the Observatory was *gr.* 50. which I know to be our *gr.* 53. So that the comparison is easy in the table at No I. IV.

4. But I know not many observations made with this Weather-glass. It is much more to our purpose to know the construction of that very remarkable one of Mr. De la Hire, that has stood in the Royal Observatory at Paris above these threescore years, and by which a register of the weather has been constantly kept. And yet we cannot but regret that neither Mr. De la Hire himself, nor any of those that succeeded him in that office, have been at the pains exactly to determine any two

* Mem. Acad. Sc. 1666. — 1698. VII. p. 835.

† Mem. Acad. Sc. 1703. p. 53.

points of it, so as that we may be enabled to imitate it; or compare it with other Thermometers; though I hope we shall be able to trace it pretty near the truth. We are expressly told, that the spirit in it always stands at *gr.* 48. in the cave of the Observatory *, corresponding by this to *gr.* 53. in Fahrenheit's. And when it freezes in the fields they speak of their Thermometer standing in the open tower as pointing at from *gr.* 30. to *gr.* 32 †: something below which the real degree of freezing would be. And from some concurring circumstances and observations I reckon this would fall to about *gr.* 28. corresponding to our *gr.* 32. as in the table No I. V. We have an observation of Mr. De la Hire himself, whereby we find that his *gr.* 28. corresponded with *dig.* 51. *lin.* 6. in Mr. Amontons's Thermometer ‡.

5. Mr. Amontons made a fine step to settle an universal Thermometer, had it not been for some inconveniences of which we had occasion to take notice formerly ||. In his the freezing point was at *dig.* 51. *lin.* 6. where our *gr.* 32 is; and the heat

* Mem. Acad. Sc. 1700. p. 8. 1701. p. 10. 1702. p. 5. 1703. p. 3. 1704. p. 2. 1705. p. 4. 1706. p. 3. 1707. p. 2. 1708. p. 61. 1709. p. 3. 1710. p. 141.

† Ibid. 1702. p. 5. 1704. p. 4. 1705. p. 4. 1706. p. 3. 1707. p. 2. 1708. p. 62. 1709. p. 3. 1710. p. 141. 1711. p. 2.

‡ Mem. Acad. Sc. 1710. p. 142.

|| Eff. I. §. 10.

of boiling water at *dig.* 73. where Fahrenheit's is at *gr.* 212: whence they can easily be compared together, as in the table No. I. VI.

6. *The Marquis Poleni gives us the result of a vast number of excellent and well-made meteorological observations, in which the heat and cold are measured with a Thermometer constructed in Mr. Amontons's way. But, as his numbers are considerably different, I thought it well worth while to give a scheme too of his Thermometer in the table at No. VII.

7. Mr. de Reaumur was very sensible of the difficulties of Mr. Amontons's construction, and proposed a new and more certain way, as he thought, of making Thermometers. He determined to begin his scale at freezing water, and at boiling water marking *gr.* 80, dividing the intermediate space of the tube into so many equal parts. I have on another occasion† given my reasons for thinking he was in some mistake with respect to both these two periods of heat. But, as many very curious and useful observations have been made with his Weather-glasses, it will be of singular use to find out the correspondence of his scale with that of Fahrenheit. His boiling-water heat is really only the boiling heat of weakened spirit of wine, coin-

* Phil. Transf. 421. p. 205.

† Ess. I, § 20, 21.

ciding nearly, as I guess, with Fahrenheit's *gr.* 180, and as his *gr.* $10\frac{1}{4}$ is the constant heat of the cave of the Observatory*, or our *gr.* 53, thence I find his freezing point, instead of answering just to our *gr.* 32, to be something above *gr.* 34, as in the table No. I. VIII.

8. On principles very like those of Mr. de Reaumur, Mr. de l'Isle constructed his mercurial Thermometer: but, instead of freezing cold, he began his scale at the heat of boiling water, and, inverting the common order, marked downwards the several degrees according to the condensations of the contained quicksilver, and consequently by numbers increasing as the heat decreased. This, as I observed†, was not in all glasses so uniform as were to be wished. But as in Mr. de l'Isle's own standards the freezing point is near to his *gr.* 150, coinciding with our *gr.* 32, it is easy to compare observations by them with the degrees of heat marked in ours by the table at No. I. IX.

9. Mr. Crucquius‡ in Holland has made many curious and accurate meteorological observations with an air Thermometer. In freezing water the whole volume was 1070, in boiling water 1510;

* Mem. Acad. Sc. 1730. p. 503.

† Ess. I. § 28.

‡ Phil. Trans. N. 381. p. 4.

so as to render it very easy to collate his Thermometer with Fahrenheit's by the table No. I. IX.

10. The most common Thermometers in England are those made (and commonly very carelessly made) after the standard one kept in the Royal Society: and many fashioned on this plan, by order of the Society, have been sent into foreign parts to establish a correspondence of observations concerning the weather in different countries: for which reason it is the more incumbent on us to find out the principles of its construction, so as to enable us to compare it with other Thermometers. The scale begins, or *gr. 0* is marked at the top, I know not well upon what grounds, and thence the numbers increase downwards as the heat decreases. * In it it is said to be *extreme hot* about the top of the scale, *hot* at *gr. 25*, *temperate* at *gr. 45*, and *gr. 65* is marked as the *point of freezing*. But, by trials made with some Thermometers that had been adjusted pretty exactly with the standard one in the Society house, I found that the spirit fell to about *gr. 78* or *79* in thawing snow; near 14 degrees lower than what had hitherto been reckoned: and this increases the wonder still more, how Dr. Cyrilli† should have found the freezing point at Na-

* Phil. Transf. N. 429. p. 103. N. 433. p. 337. 339. N. 434. p. 407.

† Ibid. N. 424. p. 336. N. 434. p. 407.

ples so high as *gr.* 55, if his Thermometer was truly adjusted to the standard.

But, to compare this Thermometer with those of other people, it is necessary to find out where they correspond in some other period of heat. What is marked *hot* or *temperate*, &c. on it, is indeed very equivocal. But, to put an end to the uncertainty, I tried and found by experience, that *gr.* $34\frac{1}{2}$ answered to *gr.* 64 in ours: from whence their agreement is easily determined for all the rest of the scale as in the table at No. I. XI.

11. Sir Isaac Newton* saw very well how vague and uncertain all the Thermometers in common use were; and therefore he contrived a new one, which I am surprised has gained so little ground in the world, as it is so simple and so easily imitated. You but begin your scale at freezing, and the distance from that to the heat of boiling water you divide into 34 equal parts or degrees, which, as they are large, may be subdivided at pleasure. How such a Thermometer answers to Fahrenheit's is seen at No. I. and XII.

12. There is a Thermometer in pretty frequent use in England, wherein they conceive the middle temperature of the air as neither hot nor cold, which therefore they mark *gr.* 0, and number both above and below, denoting by this means, as they

* Phil. Transf. No. 270. p. 824.

conceive, the degrees of heat and cold of the ambient medium. It is Thermometers graduated in this way which they commonly have in their stoves and green-houses for adjusting the respective degrees of heat the various tender exotic plants require for maintaining their life and vigour. I do not believe that these Thermometers are made upon any regular or fixed principles. They commonly indeed mark freezing at *gr.* 30 under *gr.* 0. But in Mr. Fowler's regulator, who furnishes most of the green-houses with them, he, on my desiring him to try it, found, that in melting snow the spirit fell to about *gr.* 34 under 0, and I found by some comparisons I made, that his *gr.* 16 coincided *q. p.* with our *gr.* 64: so that the whole correspondence of this with Fahrenheit's Thermometer is easily determined, as in No. I. XIII.

13. Dr. Hales* thought fit to employ a new construction of a Thermometer in making his curious experiments. He began his scale or lowest degree at freezing, or our *gr.* 32, and carried it up to *gr.* 100, which he marked where the spirit stood when the ball was heated in hot water on which wax swimming first begins to coagulate. In such water I found Fahrenheit's Thermometer point at *gr.* 142; by which the two Thermometers might easily be compared. But by experience his *gr.* 100 falls considerably above our *gr.* 142. The

* Veg. Stat. p. 37.

48 THE COMPARISON OF, &c.

result, from collating the two Thermometers together, is at No. I. and XIV. At a spring-head the doctor found his to point at *gr.* 13, while ours stood at *gr.* 48.

14. At Edinburgh they have for these many years kept a very exact register of the weather, part of which is already published in their Medical Essays: for which it is well worth while to find out its relation to other Thermometers; which, from what the authors tell us, is easily done: for, * when the bulb was immersed in melting snow, the spirit stood 8'2 inches high, the heat of the human skin raising it to 22'2 inches; so that the intermediate 14 was divided into inches and tenths of inches. And the heat of that person who graduated it I found to be *gr.* 97 in mine: from whence the comparison at any intermediate degree of heat is very easy, as in No. I. XV.

15. We have heard of many other Thermometers, and of observations and registers of weather kept by them. But they have been generally so ill limited and described, that they are of no manner of use, and, to whatever purpose they might serve their authors, are to us as if they never had been.

* Edinb. Med. Ess. I. p. 8.

E S S A Y

ON THE

HEATING AND COOLING

OF

B O D I E S.

E

ST. ANDREWS, 1739.

E S S A Y

ON THE

HEATING AND COOLING

OF

B O D I E S.

SUCH is the nature of Fire and Heat, that contiguous bodies of various degrees of warmth do naturally come to the same temperature, if inert and left to themselves without any other intrinsic principle heating or cooling the one more than the other: the hotter body losing, and the cooling one gaining some degrees of heat, till they all arrive at an *equilibrium*. Thus all inert bodies placed in our air at first heat or cool the particles next to them; but in process of time they come all to be of the same temperature with it. See *Boerh. Chem. I. p. 187, 188, 200, 281. Musschenbr. Ess. de Phys. § 961.* The times of bringing about such effects, or reducing immersed bodies to the

E. 2.

state.

state of the ambient *medium*, being different, according to their relative bulks, densities, figures, particular natures, &c.

2. A body very hot placed in a colder *medium* cools gradually, at first faster and afterwards slower, as any one would expect; and is fully confirmed by Dr. Musschenbroek's experiments*. But he did not attempt to shew after what rule, or if it was after any regular manner that such a cooling was effected. He only affirms† in general from them, that given quantities of heat are lost at first in shorter times, and gradually longer and longer, till the cooling be very slow, and at last the heat of the body coincide with that of the ambient air.

3. Sir Isaac Newton's ‡ view of this matter is vastly pretty, though not thoroughly understood by Mr. Amontons§. Having heated a lump of iron red hot, and with all the necessary precautions observed the degrees and times of the diminution of its heat when exposed to the cold air, he supposed, and that not without a great shew of reason, that while it was cooling the quantities of heat lost in given small times would always be proportional to the heats subsisting in it; reckoning the heat just

* Tent. Exp. Ac. Cim. Add. II. p. 47—57.

† Ibid. p. 52.

‡ Phil. Trans. Abr. IV. 2. p. 3.

§ Mem. Ac. Sc. 1703. p. 238.

to be the excess whereby it is warmer than the ambient air. So that from the beginning of its greatest heat to the end or time when it came to the temperature of the ambient air, taking the times in an *arithmetical* progression, the heats of the iron above the heats of the air, and the decrements of these heats will be *continually* proportional. For if there be a series of quantities proportional to their differences, these quantities and differences will be in a *geometrical* progression*. Whence the diminution of heat in such a body is like the retardation of a projected body endowed with a certain originally impressed force, and moving in a medium with a resistance always proportional to its velocity. And the progress of that decrease of heat may be determined by what is commonly demonstrated of bodies resisted in that manner†.

4. This, I say, is a most ingenious way of finding out and determining those very great and burning heats which no ordinary Thermometer can bear. But then it is not to be admitted without some restriction. The hypothesis is more mathematical than physical. It gives a fine and beautiful, but not a true view of nature. The heat of a body does not really decrease exactly in that proportion.

* Newton. Princip. II. Lem. 11. p. 230.

† See Newton. *ibid.* prop. 2. Varign. in Mem. Ac. Sc. 1707. p. 504, &c.

For were that truly the case, the body, though continually cooling, would take an infinite time to arrive at the temperature of the surrounding medium*. Which however, in fact, we find to be accomplished in a very moderate space of time. And that sooner or later according to the original heat, size, density, or other circumstances of the heated body and cooling medium.

5. If you immerse a heated body in a cold air, or other cold *medium*, that body will acquire a sort of atmosphere involving it all around, something warmer than the rest of the air, though a great deal colder than the body itself: so as that the ambient cold cannot affect the body, so much as if it were quite naked and divested of its atmosphere. But as this atmosphere is warmest in or near the beginning, and gradually cooling along with the body, the ambient coldness will have a greater proportional influence towards the end than in the beginning. Whence, though the body in whole takes a longer time to cool, the differential decrements will be in a proportion somewhat greater than its inherent quantities of heat.

6. But what way shall we determine the law of this decrease, and how far it may deviate from the general and more simple law of those decrements

* See Varign. *ibid.* p. 507, 509.

being proportional to such inherent quantities of heat? I will not insist on any speculation as sufficient to lead us to this. But we may trust to experience as our best guide. Observations carefully made, and judiciously applied, will undoubtedly bring us into the right path, or at least very near it. Now, as the result of many observations, I find that the decrements of heat may be looked on as partly equable, and partly in proportion to the subsisting heats; reckoning, as in the former hypothesis, these heats to be the excesses whereby the heat of the body is warmer than the ambient air. And consequently from the beginning to the end, taking the times in an arithmetical progression, those decrements may be resolved into two serieses. In the one, and that for ordinary of much the greatest consequence, they are always in proportion to the heats themselves, and so in a geometrical progression, as in the general theory: while in the other series, indeed the least material, the decrements are as the times, or always uniform, that is, equal quantities of heat lost in equal times. And so the law of this decrease of heat coincides with the law of the retardation of a heavy body ascending perpendicularly in a medium which resists it in proportion to its velocity*.

* See Newton. *ibid.* pr. 3. Varign. *Mem. Ac. Sc.* 1708. p. 175, &c.

7. To illustrate this by observation, I chose chiefly to employ Dr. Musschenbroek's experiments; as having been carefully enough done, and not liable to the objection that might be started against mine, as if they had been made with a design to be subservient to particular rules and theories. Though indeed it was the observations that led me to think of any such rules taking place.

In a small bar of Iron. Musschenbr. Tent.

Acad. Cim. Add. II. p. 48.

The Times.	Degrees of Heat lost, by obser- vation.	Degrees of Heat lost, by the first conside- ration.	Degrees of Heat lost, by the 2d conside- ration.	Degrees of Heat lost, ac- cording to cal- culation	The Differ- ences.
1	62'5	59'4	1'2	60'6	-1'9
2	106	99	2'4	101'4	-4'6
3	128'6	125'7	3'6	129'3	+0'7
4	148'8	143'6	4'8	148'4	-0'4
5	161'8	155'6	6	161'6	-0'2
6	170'4	163'6	7'2	170'8	+0'4
7	176'5	169	8'4	177'4	+0'9
7½	180	171	9	180	0

In a small bar of Steel. *Ibid.* p. 49.

1	61'4	56'1	0'5	56'6	-4'8
2	94	95'2	1	96'2	+2'2
3	120	122'4	1'5	123'9	+3'9
4	141'5	141'4	2	143'4	+1'9
5	158'4	154'6	2'5	157'1	-1'3
6	167'7	163'8	3	166'8	-0'9
7	174'7	170'2	3'5	173'7	-1
8	179	174'7	4	178'7	-0'3
9	182'6	177'8	4'5	182'3	-0'3
10 q. p.	185	180	5	185	0

In

In a small bar of Copper. *Ibid.* p. 50.

The Times.	Degrees of Heat lost, by obser- vation.	Degrees of Heat lost, by the first confide- ration.	Degrees of Heat lost, by the 2d confide- ration.	Degrees of Heat lost, ac- cording to cal- culation	The Differ- ences.
1	76	71'4	0'8	72'2	-3'8
2	124'5	115'8	1'6	117'4	-7'1
3	141'2	142'2	2'4	144'6	+3'4
4	160'5	158'7	3'2	161'9	+1'4
5	171'6	168'8	4	172'8	+1'2
6	179'2	175'1	4'8	179'9	+0'7
7	184'4	178'9	5'6	184'5	+0'1
7 ² / ₅	185	179'3	5'7	185	0

In a small bar of Brass. *Ibid.* p. 51.

1	120	114'1	1'5	115'6	-4'4
2	187	185	3	188	+1
3	232'6	228'7	4'5	233'2	+0'6
4	260'8	255'8	6	261'8	+1
5	280'2	272'6	7'5	280'1	-0'1
6	291	283	9	292	+1
7 q.p.	300	289'5	10'5	300	0

In a small bar of Lead. *Ibid.* p. 51.

The Times.	Degrees of Heat lost, by obser- vation.	Degrees of Heat lost, by the first con- fide- ration.	Degrees of Heat lost, by the 2d con- fide- ration.	Degeees of Heat lost, ac- cording to cal- culation	The Differ- ences.
1	125	114	0'1	114'1	-9'9
2	177	174'6	0'2	174'8	-2'2
3	208	207	0'4	207'4	-0'6
4	225	224'3	0'5	224'8	-0'2
5	234	233'5	0'6	234'1	+0'1
6	239'1	238'4	0'7	239'1	0
7	241'7	241	0'8	241'8	+0'1
8	243'2	242'4	0'9	243'3	+0'1
8 $\frac{3}{4}$	244	243	1	244	0

In a small bar of Tin. *Ibid.* p. 52.

1	61'4	61	0'5	61'5	+0'1
2	96'6	96'2	1	97'2	+0'6
3	120	118'5	1'5	118	-2
4	132'7	128'1	2	130'1	-2'6
5	139'1	134'8	2'5	137'3	-1'8
6	142'4	138'7	3	141'7	-0'7
6 $\frac{4}{5}$	144	140'6	3'4	144	0

In

In a little Mercurial Thermometer, heated to *gr.* 108, and exposed to the open air, I found the heat decrease in this order.

The Times.	Degrees of Heat lost, by obser- vation.	Degrees of Heat lost, by the first confide- ration.	Degrees of Heat lost, by the 2d confide- ration.	Degrees of Heat lost, ac- cording to cal- culation	The Differ- ences.
0 $\frac{1}{2}$	13	13'6	0'4	14	+1
1	25	24'1	0'8	24'9	—0'1
1 $\frac{1}{2}$	34	32	1'2	33'2	—0'8
2	40	38'1	1'6	39'7	—0'3
2 $\frac{1}{2}$	45'5	42'8	2	44'8	—0'7
3	49'5	46'4	2'4	48'8	—0'7
3 $\frac{1}{2}$	52'5	49'1	2'8	51'9	—0'8
4	55	51'2	3'2	54'4	—0'6
4 $\frac{1}{2}$	56'5	52'8	3'6	56'4	—0'1
5	58	54	4	58	0

8. The decimals of degrees are calculated and set down as they arose in the arithmetical reduction of the numbers: Not that I would be thought to pretend to an imaginary exactness either in my experiments, or those copied from Dr. Musschenbroek. Measures and observations of this kind admit but of an approximation to geometrical truth. All our machines are imperfect. The construction of Dr. Musschenbroek's Pyrometer made with toothed wheels, pinions,

pinions, &c. could not, as he himself confesses *, move with such a regularity as were to be wished; and perhaps too the heated or cooled bodies are not quite homogeneous †, and so sometimes make the refrigerations go on in starts: and accordingly in the register of his observations we meet with starts, stops, and irregularities, that in such compound engines and imperfect bodies are unavoidable. And so we are to take the medium and main drift of the observations, in the main agreeing very well with the theory, though they may differ some degrees on one side or the other.

9. From this law of the decrease of heat in bodies exposed to the influence of an equable stream of cooling air we shall be able with a tolerable degree of exactness to find out the intensity of those great burning fiery heats which cannot be measured by any Thermometers yet known. The heat of metals red hot, or even in fusion, of burning stones, melted glass, &c. may by this means be determined. For when they are brought to the proposed heat by a strong fire we are carefully to observe the times of their cooling; and by applying a mercurial Thermometer to them as soon as it can well bear their heat, we may accurately enough determine the decrements of heat below that period; and from thence, rising upwards in a regular series,

* Tent. Exp. Acad. Cim. Add. II. p. 29.

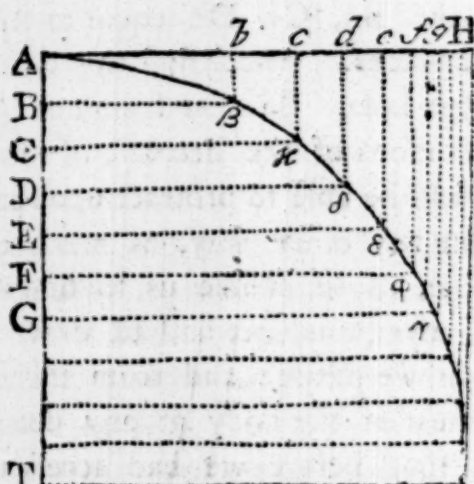
† See Ibid. p. 30, 31, 32.

be able to find out the degrees of heat from that to the very beginning of its cooling, or the moment it was taken from the fire.

10. I am very well aware that this law I have proposed of the decrease of heat will not always hold exactly in all circumstances. If the heated body be very large, and without a free ventilation of air, it will acquire a warm and almost stagnating atmosphere around it, which increases and spreadeth wider for a considerable time after the beginning of the observation, and then gradually is diminished and drawn into nothing at the end or expiration of heat. By which means it neither cools so fast, nor in such a regular manner, as if a constant brisk stream of fresh air were perpetually blowing along it. In those circumstances it will be difficult, if not impossible, to reduce the decrease of heat to a regular series of numbers; however even in this case, as well as in the former more regular decline of heat, we may be able to find a method of tracing its several degrees in all its different periods of time, and exhibit them to the eye as distinctly, or rather more so, than if they were expressed in numbers; and that too in a sort of geometrical way.

11. Let

17. Let the time the body takes in cooling be expressed by



the line AT; the quantity of heat lost (being its excess above the heat of the ambient medium) by AH drawn perpendicular to it. AT be-

ing divided into any number of very small equal parts AB, BC, CD, &c. gives AB, AC, AD, &c. the times from the beginning increasing in an arithmetical progression. And in AH the lines Ab, bc, cd, &c. are supposed to express the decrements of heat in these respective small times. So that Ab, Ac, Ad, &c. are the quantities of heat lost from the beginning of the observation at any assigned moment of time, and HA, Hb, Hc, &c. the heats still subsisting at the respective times A, b, c, &c. Now from B, C, D, &c. draw the lines B β , C κ , D δ , &c. parallel to AH: and from b, c, d, &c. draw b β , c κ , d δ , &c. parallel to AT, so that they meet respectively, in β , κ , δ , ϵ , &c. and through these points of concurrence draw the curve A β κ δ ϵ , &c. this curve line, be-

ing concave towards AT and convex towards AH will be the curve of the decrease of heat. AT may be lookt on as its axis, AB, AC, AD, &c. as the abscisses, and B β , C γ , D δ , equal to Ab, Ac, Ad, as its ordinates. And I find this curve to be a sort of hyperbola. So that from any given series of observations of the decrease of heat of any body we shall be able to protract upon paper, or to calculate any other way, as much of this hyperbolic line, as to enable us to find its center, diameters, asymptots, &c. and to draw it out to what length we please: and from thence to investigate the heat of the body at any determined distance of time before we had access to know the heat by a Thermometer. By which method then, only from observing the decrements of heat in a lump of iron, or any other intensely hot body, after it is able to bear the application of a Thermometer, having taken particular notice how soon it comes to that temperature; we may determine its degree of heat the moment it was taken burning hot from the furnace. We may either calculate backward the series of numbers found out by observation, or protract on paper mechanically the hyperbolic line or scale of the decrease of its heat in the manner just now proposed. For either of these ways will give us the initial greatest heat it had when taken from the fire with tolerable exactness. The former will be sufficient, if the hot body be very small, or a stream
of

of cool air be constantly blowing along it; but the other will be more convenient if the surrounding air be in a state of stagnation or nearly so.

12. We see then from the first moment of the cooling of bodies the quantities of heat lost in given times are always growing less and less; till the heat and consequently these decrements of heat quite evanish, and the body put on the temperature of the ambient medium; and all this in a pretty regular manner. The heating of bodies does not go on in such an orderly way; concerning which we have some very nice experiments furnished us by the ingenious and accurate Professor Musschenbroek*: from which he justly infers, that the expansion or heating of bodies is not equable, but very slow at first, as if the fire had some difficulty in its first penetration of the body: which gradually grows easier, the expansions or increments of heat in given times soon coming to be very considerable. These in a very little time come to their greatest height. And then such increments gradually diminish till they quite evanish, the heated body acquiring the temperature of the ambient medium†. All which I have likewise found to be true by the experiments I have made on this sub-

* Tent. Exp. Acad. Cim. Add. II. p. 24—28. 34—43.

† Ibid. p. 29—32. 39.

ject. Some of which I shall soon have occasion to insert.

13. That bodies cool or heat sooner or more leisurely according to their different bulks, densities, figures, &c. is commonly taken notice of. There will be no great labour required to convince every man that will but think of it, that a given quantity of the same sort of matter will be easier or more difficultly cooled or heated, as it is expanded under a greater or smaller surface; and consequently must vary according to the different figures wherein it is shaped. Thus bodies of a round form as having the greatest quantity of matter within the least surface are, *ceteris paribus*, the slowest in heating or cooling, and so on of the other figures of bodies, according to their compactness or expansion.

12. As to their difference of size, no body makes any doubt of great bodies altering their temperature more slowly than smaller ones: "Quo
" minor est corporis moles, says Lord Verulam *,
" eo citus per corpus calidum approximatum in-
" calefcit." But in what proportion to their bulk is not altogether so easy to determine. Let us however hear what Sir Isaac Newton †, says on the subject. "Globus major calorem diutius confer-

* Nov. Org. II. 13. § 40.

† Princ. Math. p. 509.

“ varet in ratione diametri, propterea quod superficies (ad cujus mensuram per contactum aëris ambientis refrigeratur) in illa ratione minor est pro quantitate materiæ suæ calidæ inclusæ.— Suspicor tamen quod duratio caloris, ob causas latentes augeatur in minore ratione quam ea diametri: & optarim rationem veram per experimenta investigari.”

15. To satisfy this great man's wishes, I began to make some experiments of this sort, which I proposed to have carried on farther if I had had leisure enough. However, from the trials I made, I think we have good reason to conclude his speculation of the faculties to preserve heat being proportional to the diameters of similar bodies, to be very just: and that his suspicion of that faculty in the greater bodies, not being fully in such a high proportion as the diameters, is without sufficient ground.

16. I took two China bowls pretty nearly of the same shape but of very different sizes, the large one being about twice as broad, and so of the other longitudinal dimensions, as the other, and consequently holding eight times more liquor. Into the big one A I put two quarts, and into the other B two gills of water, which in both was brought to the temperature of *gr.* 112. by very nice Thermometers placed in each, and immediately removed into

into a cool place, where there was a moderate current of air of the temperature of about *gr.* 48 or 49, and the refrigerations of the waters carefully observed in the following times.

Degrees of Heat.	Times in A.	Times in B.	
		By observa- tion.	By theory.
<i>gr.</i> 112	0	0	0
104	$10\frac{1}{2}$	$5\frac{1}{2}$	$5\frac{1}{4}$
96	$21\frac{1}{4}$	11	$10\frac{5}{8}$
80	$54\frac{1}{2}$	28	$27\frac{1}{4}$
72	81	42	$40\frac{1}{2}$
64	118	61	59
56	194	99	97

The same day the experiment was tried over again, but in a place where the surrounding air had no current, and little other agitation than what was occasioned by the motions I made in carrying on the observations. The result was as follows:

Degrees of Heat.	Times in A.	Times in B.	
		By observa- tion.	By theory.
<i>gr.</i> 112	0	0	0
104	10	6	5
100	16	9	8
96	23	$12\frac{1}{2}$	$11\frac{1}{2}$
92	31	16	$15\frac{1}{2}$
88	40	21	20

The

The Thermometer in the little bowl was somewhat above *gr.* 112 in the beginning, which was the cause of the times of the falling to such and such degrees of heat to be always about a minute longer than the theory required. There was something, though less of this too, in the foregoing experiment: and, making a due allowance for these small and truly inconsiderable variations, the theory coincides surprisngly with observation; nearer indeed than we could well expect in such experiments, wherein the instruments must be allowed not to come up to a mathematical exactness. A very small and insensible error in the construction of the Thermometers, or a very inconsiderable mistake, as of about a quarter or half a degree in making the observation, would seemingly occasion a very sensible difference between the time actually observed and that which the theory required.

To try it in bodies differing still more in bulk from one another, the heat of the air being *gr.* 48, there were two vessels filled, the one with one gill and the other with 27 gills of water, their respective diameters thus being 1 and 3; and the refrigerations came out in this order.

Degrees

Degrees of Heat.	Times in A.	Times in B.	
		By observa-	By theory.
	' "	' "	' "
gr. 108	0 00	0 : 00	0 : 00
104	5 : 20	1 : 30	1 : 46
100	10 : 00	3 : 20	3 : 20
96	15 : 45	5 : 05	5 : 15
92	22 : 10	7 : 10	7 : 23
88	30 : 00	9 : 00	10 : 00
84	39 : 00	11 : 15	12 : 20
80	48 : 20	14 : 00	16 : 07
76	57 : 00	17 : 30	19 : 00
64	101 : 00	33 : 10	33 : 40

There should be more experiments tried, and these on other bodies as well as water; on solids as well as fluids, and in a greater variety of proportions of their respective bulks, and with bodies immersed in various mediums, &c. all which I am apt to think would come out, like to what we have already tried, agreeable to Sir Isaac's rule.

17. Let us next inquire if the ordinary theory of bodies heating and cooling faster or slower, as they are less or more dense, will come out as conformable to nature and truth. It is indeed a very plausible speculation, and what one would be apt to deduce from the *vis inertiae* of matter, that rare bodies are soon either heated or cooled, but that dense bodies are more slowly altered by the heating

or

or cooling influence of the ambient medium.
 “ *Suspīcamur (naturam densi & rari) posse habere consensum cum tarda & celeri exceptione & depositione calidi & frigidi. Fiat igitur experimentum, si rarius corpus non admittat, & amittat calorem aut frigus celerius, densius vero tardius.”—“ Quo densiora corpora,” says Dr. Boerhaave †, “ siue fluida fuerint siue consistentia, eo pluri tempore egent, ut ab eodem igne æqualiter incalescant.” And in like manner, as to the cooling of bodies, he tells us ‡, “ Corpora—acceptum caloris gradum tanto diutius conservare quo sunt densiora, ponderosiora, aut plus substantiæ corporeæ habentia.” In a word, “ Quo densa magis corpora eo diutius impressi calidi tenacia ||.—Tanto pluri tempore egent ut possint redire ad temperiem cum rarioribus citius refrigerandis ***.”

This rule of bodies requiring times of heating and cooling in the order and proportion of their densities, is, I say, a very plausible speculation, and to which every body at first hearing is ready to assent: and moreover this proposition, so obvious in theory, is said to be confirmed too by experi-

* Bacon Hist. Denf. &c. p. 17.

† Chem. I. p. 279. vid. & p. 266.

‡ Ibid. p. 264.

|| Ibid. p. 160.

*** Ibid. p. 267.

ments, as we find them proposed by Dr. Boerhaave*,
 “ Multa super hac re cogitanti” (says he) “ *obser-*
 “ *vatum* certe id fuit, eo citius calefcere ab eodem
 “ igne corpus quo, rarius fuerit; eo lentius autem
 “ refrigerari calefactum semel, quo densius fuerit,
 “ eo citius refrigerfcere quo rarius erat. Si vas aëre
 “ plenum, aliud aqua, utrumque dein eidem calo-
 “ ri exponitur, erit forte aër sic calidus millies ra-
 “ rior aqua quoque ita calida, sed aqua conceptum
 “ tanto tardius calorem tanto diutius retinebit, ut
 “ aër forte millies citius refrigerfcatur.—† Sit vas ca-
 “ vum, parallelepipedum, supra apertum, ex aëre,
 “ aqua repletum; cui imponantur vasa cylindrica
 “ vitrea, æqualia; repleta ad eandem altitudinem
 “ diversis pondere liquidis; dein supponatur ignis,
 “ ut aqua intra hoc vas assidue mota æquabilif-
 “ sime incalcfcat, cernemus nudo oculo liquidum
 “ livius, adeoque rarius, citissime expandi, den-
 “ sius vero multo lentius; quin & thermoscopia
 “ imposita idem docebunt. Calefcit ocysfime aër,
 “ dein Alcohol, Oleum Petrolçi liquidiffimum po-
 “ ftea, tum Oleum Terebinthiæ, mox aqua pura,
 “ dein aqua falsa, Lixivium fortiffimum, Metal-
 “ la, Mercurius, Aurum.” And, as to their cool-
 ing, he gives us a similar experiment, and with a
 like fuccess. “ ‡ Si enim in aqua ebulliente diver-
 “ fa pondere corpora æqualiter inde calefcunt,

* Chem. I. p. 200.

† Ibid. p. 279.

‡ Ibid. p. 264.

“ id diutissime calidum manebit quod ponderosissi-
 “ mum, id citissime friget quod leve. Quosque
 “ autem hætenas per experimenta procedere datur
 “ regula fere generalis hæc habetur; vacuum Tor-
 “ ricellianum calorem in eo genitum una momento
 “ amittit. Aër calefactus in olla, citissime calorem
 “ conceptum perdit. Alcol lentiore gradu. A-
 “ qua diutius quam Alcol. Argentum Vivum tar-
 “ dius vero eodem calore refrigeratur. Ita inter
 “ solida quoque, lignum, saxum, metella, iterum
 “ equaliter calefacta, retinent quesitum calorem
 “ exacte tanto serius.” This scheme of bodies
 having a faculty of preserving their temperature in
 proportion to their respective densities, is illustra-
 ted from fundry considerations by Dr. Musschen-
 broek *, who reckons them up in this order †.
 air, alcohol, petroleum, oil of turpentine, rape-
 seed oil, distilled vinegar, water, salt water, aqua
 fortis, oil of vitriol, spirit of nitre, quicksilver.

18. I know no stronger instance than this of the
 weakness, or, if I may venture to say so, of the
 presumptuousness of the human understanding, in
 pronouncing too hastily concerning the nature of
 things from some general preconceived theories.
 That rule of bodies acquiring given degrees of heat
 or cold, *cæteris paribus*, in times just reciprocally

* Ess. de Phys. § 969.

† Ibid. § 944.

proportional to their densities, though plausible enough in theory, I cannot find to be true in fact. Air, indeed, is heated and cooled sooner than any other fluid I have had opportunity of trying, though not in such a different proportion as its extreme rarity, compared with the great density of other fluids, would require. And water is slower of heating and cooling than either oil or alcohol: but, contrary to all our fine theory, quicksilver, the most dense ordinary fluid in the world, excepting only melted gold, is however the most ticklish next to air; it heats and cools sooner than water, oil, or even rectified spirit of wine itself.

Taking about 15 ounces of mercury, and $\frac{1}{14}$ of this weight of Water, so that their volumes were pretty equal, I put them into two very thin glasses of the same size and make, with a Thermometer in each: and after letting them stand a good time in the cold air, till both Thermometers pointed precisely *gr.* 46, I set them down at the same time almost close to one another, before a great fire, at a considerable distance from it, but so that the heat should equally act upon them. And in the following times the Thermometers (which in themselves were very like one another, and equally ticklish) shewed the respective fluids to be heated in the following order.

Times

Times.	Water heating.	Mercury heating.
0 or beginning.	gr. 46	gr. 46.
3	51	56
6	57	66
9	62	73
12	66	78
15	70	82
18	73	84
21	76	86
24	77 $\frac{1}{2}$	86 $\frac{1}{2}$
27	79	87
30	80	87
33	80 $\frac{1}{2}$	87
36	81	87
39 The fire	82	88
42 stirred.	84	90
45	85	92
48	87	93—
51	87 $\frac{1}{2}$	93+
54	88	93+
57	88 $\frac{1}{2}$	93+
60	89	93.

Then the water and quicksilver being both brought to the same temperature of gr. 59, were at the same time set down in an open window, where the temperature of the air was at gr. 52; and the two Thermometers shewed the fluids to cool in this order:

Times.	Water cooling.	Mercury cooling.
0 or begin-	gr. 89	gr. 89
3 ning.	$85\frac{1}{2}$	$81\frac{1}{2}$
6	82	76
9	79	72
12	76	68
15	$73\frac{1}{2}$	65
18 A blast	$70\frac{1}{2}$	$61\frac{1}{2}$
21 of air.	68	$59\frac{1}{2}$
24	66	58
27	$64\frac{1}{2}$	57
30	63	56
33	62	$55\frac{1}{2}$

Here we see that water, instead of acquiring or losing a given quantity of heat 13 or 14 times sooner than quicksilver, as it should do by the common theory, is about twice so slow in heating and cooling than what mercury was found to be. Could the glasses have been made any thinner, perhaps the mercury would have appeared to cool still something more quickly. And as the fire did not act equally all the while, you find too some little anomalies occasioned thereby.

As this experiment differed so widely from the common opinion, and from what every body, to whom I have had occasion to propose it, expected, I tried it over again with the same cautions, but with larger volumes, taking near 48 ounces of mercury, and $3\frac{1}{2}$ ounces of water; their quantities

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ties being to one another as $13\frac{1}{2}$ to 1; and so their volumes pretty equal. And the result of the observation was as follows.

Times.	Water heating.	Mercury heating.
0 or beginning.	gr. 48	gr. 48
4	52	58
8	$57\frac{1}{2}$	69
12	63	78
16	68	$84\frac{1}{2}$
20	$72\frac{1}{2}$	$89\frac{1}{2}$
24	77	$92\frac{1}{2}$
28	80	95
32	83	$96\frac{1}{2}$
36	85	97
40	87	98
44	89	99
48	91	100
52	$92\frac{1}{2}$	100
56	$93\frac{1}{2}$	100
60	94	$99\frac{1}{2}$

They were both reduced to the heat of gr. 92, and then carried into the air of the temperature of gr. 51, and cooled in the following order; all the while the water evaporating a little, and sticking in drops to the side of the glass.

Times.	Water cooling.	Mercury cooling.
0 or begin-	gr. 92	gr. 92
4 ning.	88	$85\frac{1}{2}$
8	85	$80\frac{1}{2}$
12	82	76
16	$79\frac{1}{2}$	$72\frac{1}{2}$
20	77	$69\frac{1}{2}$
24	75	67
28	73	$64\frac{1}{2}$
32	$71\frac{1}{4}$	$62\frac{1}{2}$
36	$69\frac{1}{2}$	61
40	68	$59\frac{1}{2}$
44	$66\frac{3}{4}$	$58\frac{1}{4}$
48	$65\frac{1}{2}$	57
52	$64\frac{1}{2}$	$56\frac{1}{4}$
56	$63\frac{3}{4}$	$55\frac{1}{4}$
60	$62\frac{3}{4}$	$54\frac{1}{2}$
64	$61\frac{3}{4}$	54
68	61	$53\frac{1}{2}$

19. As oil is more tenacious than water, many are apt to think it will be slower both in admitting the particles of heat and fire, and in cooling or letting them go. “Omnes Philosophi,” says Dr. Boerhaave *, “conferent olei tenacitate irretitum ignem longe diutius in oleo hæsurum.” And another theory will lead them to a new opinion: when they consider that oil, consisting chiefly of sulphureous particles, will therefore, from their notion of the Newtonian observations of the rays

* Chem. I. p. 263.

of light and heat, be more apt both to attract and retain the heat applied to it. But how weak and fallacious are all our speculations concerning the nature of things not thoroughly well understood. Dr. Boerhaave, in direct opposition to this last theory, makes water both heat and cool more slowly than oil; and that exactly in proportion to their respective densities. * “*Bina vasa æqualia accepi, quorum unum aqua, oleo implevi olivæ alterum. Ambo reposui in vase, in quo feci ebullire aquam, retinui in illa ebulliente aqua, donec certus essem utrosque liquores eodem bullientis aquæ gradu æque calefactos; exemi tum ex eo vase, reposui in eodem ambo aëre, ut viderem tempus, quo uterque liquor reducebatur ad eundem refrigerationis gradum; atque inventi, pro ratione ponderis comparati, prorsus æquale.*” Nor this did I find exactly true. Oil did indeed both heat and cool faster than water: and, contrary to all the theories, even much faster than its difference in specific weight should have made it. Oil of olives is but about $\frac{1}{10}$ lighter than water: but it is heated and cooled faster in a much greater proportion. That excess is not indeed always the same, or near the same. It does not shew such an uniformity of difference as quicksilver did. In the beginning there is no great difference; then water takes about $\frac{1}{4}$ or so more time in acquiring

* Chem. I. p. 263.

or losing given quantities of heat than what the oil does : but in the progress of the experiment, the difference becomes always greater and greater, till towards the end the water is almost twice as slow in heating and cooling as oil, taking the times from the beginning of the observation ; as in the following experiments.

Times.	Water heating.	Oil heating.
0 or begin-	gr. 50	gr. 50
8 ning.	54	54
16	63	66
24	71	78
28	75	83
32	79	88
36	82	93
40	$85\frac{1}{2}$	$96\frac{1}{2}$
44	88	$99\frac{1}{2}$
48	91	102
52 the fire de-	94	105
56 caying.	96	107
60	99	109
64	101	$110\frac{1}{2}$
68	103	$111\frac{1}{2}$
72	104	$111\frac{1}{2}$
76	$104\frac{1}{2}$	$111\frac{1}{2}$

And both being brought to the temperature of gr. 108, they were set in the air of between gr. 50 and 51, and they cooled in this order :

Times

Times.	Water cooling.	Oil cooling.
0 or begin- ning.	gr. 108	gr. 108
4	104	102
8	100	96
12	$96\frac{1}{2}$	90
16	94	86
20	$91\frac{1}{2}$	$81\frac{1}{2}$
24	$88\frac{1}{2}$	$77\frac{1}{2}$
28	86	74
32	84	72
36	82	70
40	81	68
44	$79\frac{1}{2}$	$66\frac{1}{2}$
48	78	65
52	$76\frac{1}{2}$	$63\frac{1}{2}$
56	75	$62\frac{1}{2}$
60	74	$61\frac{1}{2}$
68	$71\frac{1}{2}$	60
76	$69\frac{1}{2}$	$58\frac{1}{2}$
84	$67\frac{1}{2}$	$57\frac{1}{2}$

The experiment of the heating and cooling of oil was repeated with the following success.

Times.

Times.	Water heating.	Oil heating.
0 or begin- ning.	<i>gr.</i> 45	<i>gr.</i> 45
4	45 $\frac{1}{2}$	46
8	49	54
12	52 $\frac{1}{2}$	55 $\frac{1}{2}$
16	57 $\frac{1}{2}$	62
20	61 $\frac{1}{2}$	69
24	65	74
28	68 $\frac{1}{2}$	78 $\frac{1}{2}$
32	71 $\frac{1}{2}$	82 $\frac{1}{2}$
36	74 $\frac{1}{2}$	86
40	77	88 $\frac{1}{2}$
44	79	90 $\frac{1}{2}$
48	81	93
52	83	95
56	84 $\frac{1}{2}$	96
60	86	97
64	87	98
68	88	98 $\frac{1}{2}$
72	89	99
76	91	100 $\frac{1}{2}$
80	92	102
84	93 $\frac{1}{2}$	103
88	94 $\frac{1}{2}$	103 $\frac{1}{2}$
92	94 $\frac{3}{4}$	103 $\frac{3}{4}$
96	95	104

Both the water and oil were brought to the heat of *gr.* 96, and placed in a large room, the temperature of which was about *gr.* 45; and they cooled in this order:

Times.

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Times.	Water cooling.	Oil cooling.
0 or beginning.	gr. 96	gr. 96
4	92	90
8	89	85
12	86	80
16	$83\frac{1}{2}$	$76\frac{1}{2}$
20	$81\frac{1}{2}$	73
24	$79\frac{1}{2}$	70
28	$77\frac{1}{2}$	$67\frac{1}{2}$
32	$75\frac{1}{2}$	$65\frac{1}{2}$
36	74	$63\frac{1}{2}$
40	$72\frac{1}{2}$	$61\frac{1}{2}$
44	71	60
48	$69\frac{1}{2}$	$58\frac{1}{2}$
52	68	$57\frac{1}{2}$
56	67	$56\frac{1}{2}$
60	66	$55\frac{1}{2}$
68	$64\frac{1}{2}$	54
76	$62\frac{3}{4}$	$52\frac{3}{4}$
84	$61\frac{1}{4}$	$51\frac{3}{4}$
92	$59\frac{3}{4}$	51
100	$58\frac{1}{2}$	$50\frac{1}{2}$
116	56	49
132	54	48
148	$52\frac{1}{2}$	$47\frac{1}{2}$
180	$50\frac{3}{4}$	$46\frac{1}{2}$
212	49	$45\frac{1}{2}$
244	$47\frac{3}{4}$	$45\frac{1}{4}$
276	47	45
340	46	45

20. The

20. The heating and cooling of water and rectified spirit of wine came out as follows :

Times.	Water heating.	Sp. wine heating.
0 or beginning.	gr. 52	gr. $52\frac{1}{2}$
4	53	54
8	$56\frac{1}{2}$	$59\frac{1}{2}$
12	61	$65\frac{1}{2}$
16	65	71
20	69	76
24	73	81
28	77	85
32	80	88
36	83	91
40	$85\frac{1}{2}$	$93\frac{1}{2}$
44 The fire decreasing.	88	96
48	90	$97\frac{1}{2}$
56	$92\frac{1}{2}$	$99\frac{1}{2}$
60	94	100
64	95	100

They were both brought to the heat of gr. $95\frac{1}{2}$, and exposed to the open air of the temperature of gr. 57 or 58, where they cooled in this order :

Times.

Times.	Water cooling.	Sp. wine cooling.
0 or beginning.	gr. $95\frac{1}{2}$	gr. $95\frac{1}{2}$
4	$91\frac{1}{2}$	$90\frac{1}{2}$
8	88	86
12	85	$82\frac{1}{2}$
16	$82\frac{1}{2}$	79
20	80	$75\frac{1}{2}$
24	78	73
28	76	71
32	74	69
36	$72\frac{1}{2}$	$68\frac{1}{2}$
40	71	67
44	70	$66\frac{1}{4}$
48	69	$65\frac{1}{2}$
52	68	64
68	65	62
84	$62\frac{1}{2}$	$60\frac{1}{2}$

Thus we see that spirit of wine both heats and cools faster than water, and that in a much greater proportion than the inverse *ratio* of their specific weight does require, as we observed likewise of oil. But still quicksilver, however dense, is more ticklish and easier affected by heat and cold than any of these fluids. Common brandy, upon trial, I did not find to differ sensibly from water in this respect.

21. The same theory, drawn I suppose from the general doctrine of the *vis inertiae* of matter, led

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Dr. Boerhaave * and Dr. Musschenbroek † to lay it down as a principle, that the faculty of cooling other bodies was in proportion to the density of the cooling medium. “Corpora,” says Boerhaave, “quæ ignem jam in se continent copia majore, quam ambieptia fluida, vel vicini corpora, illum ignem amittunt eo citius, quo in fluidum densius immittuntur refrigerandi causa. Quod ita intellectum velim. Sit aër, aqua, argentum vivum, in vasis, ejusdem accurate temperiei in omnibus his. Esto tum igniti ferri frustum triplex æquale perfecte ignitum. Unum horum trium relinquatur in aëre notatæ temperiei, immergatur secundum in aquam accurate tam frigidam quam aër hoc tempore. Tertium vero intrudatur in argentum vivum etiam æque frigidum ac priores, aër et aqua. Quid fiet? in raro aëre ferrum diu retinebit suum calorem, in aqua citius amittet, in argento vivo citissime. Et quidem videtur in illa aqua fere tanto refrigerari citius, quanto hæc aëre densior; adeoque occurrentes ocyus. In argento vivo forte quaterdecies citius, quam in aqua.”

22. Neither does this speculation come out true in fact. The times of bodies cooling in these different fluids do by no means differ so much as is commonly imagined. Instead of 800, a body cools

* Chem. I. p. 267.

† Ess. de Phys. § 967.

but about 8 times faster in water than in air. And the refrigerating virtue of quicksilver was found instead of 14 times greater than that of water to be a very small matter so, as about 2 seconds or so in a minute.

Air, water, and quicksilver, being all brought to the common temperature of *gr.* 52, I placed successively in these several fluids a small Thermometer that had always been heated to *gr.* 112; and carefully observed the times wherein it lost 56 degrees of heat, or wherein the mercury sunk to *gr.* 56 on the scale, which was within 4 degrees of the heat of the surrounding *mediums*. And by four different trials the refrigerations came out thus:

		' "	' "	' "	' "
In Air	-	8:45	8:30	8:20	8:25
In Water	-	1:04	1:03	1:04	1:04
In Quicksilver	-	1:00	1:03	1:01	1:02

23. This doctrine of the heating and cooling of bodies, in all its several branches, should be carried on by a variety of experiments, which I have neither leisure nor opportunity of prosecuting at this time. So that I must leave it to others, whose situation is more favourable for making the necessary observations for establishing and perfecting that doctrine, to which I have only been able to furnish some helps.

A N
E S S A Y

TOWARDS A
NATURAL and EXPERIMENTAL HISTORY
OF THE
VARIOUS DEGREES of HEAT in BODIES.

H 3

ST. ANDREWS, 1738-39.

A N

E S S A Y

TOWARDS A

NATURAL and EXPERIMENTAL HISTORY

OF THE

VARIOUS DEGREES of HEAT in BODIES.

I. *Of the way of computing the different degrees of Heat.*

MANY of the ancients had strange notions of the nature of Heat. They supposed it in different subjects to differ in kind as well as quantity. They talked very magnificently of the Celestial Heat, as differing much in its nature from the heats commonly produced on our earth. And these too they thought to be of quite different natures in the different bodies wherein they are lodged. The heat of the fire, or of hot water, or of fermenting substances, they thought of a lower

lower kind, and altogether distinct from the heat of animals. And this too they distinguished into natural and præternatural, or morbid, as sorts of heat quite different from one another. And those too they reckoned of different natures in the different species of animals. Doctrines and ways of speaking of this sort set up by the Peripatetic school, and too much adopted by Galen and the physicians after him, continued long in the world; and were also countenanced by the chymists, these *philosophi per ignem*, who professed and valued themselves on a more than ordinary knowledge of the secrets and operations of heat.

2. Gomez Pereira, who was among the first that dared to think freely in philosophy as well as physic, ventured to affirm * all heat, whether of an elementary, igneous, or animal kind, to be of one and the same nature; that the heat of the sun, or any other elementary heat, did not differ from that of animals but in degree; and that the heat of a man in a fever differed only by its excess from that of a man in health; and that the heats of other animals did not differ specifically, but only in quantity, from one another, or from that of the human kind.

* Nov. Med. cap. ii. p. 7, 9. cap. iii. p. 12, 17, 20, 23, 24, 25.

3. The learned Telesius, though he was sensible of the great variety of the degrees of heat in bodies, thought that the determining them and ascertaining their differences to be almost impossible. So that he declines taking the trouble of any such inquiry. “*Qui calor,*” says he*, “*vel quantus, & quæ ejus copia, & quæ entia in qualia invertat minime inquirendum videtur, ut quod homini nulla, ut nobis videtur, innotescere queat ratione. Quî enim vel caloris vires, & calorem ipsum veluti in gradus partiri, vel materiæ, cui inditus est, copiam quantitatemque distincte percipere, & certis determinatisque caloris viribus copięque in certam materiæ quantitatem, dispositionemque certas actiones; & certæ materiæ quantitati certam determinatamque caloris copiam assignare liceat?*”

The pursuit however of all this he recommends to others, as being in itself of very great consequence. “*Utinam id alii & perspicaciori præditi ingenio, & quibus in summa tranquillitate rerum naturam perscrutari licuerit assequantur; ut homines non omnium modo scientes, sed omnium fere potentes fiant.*” So fond would he have been of the ways of measuring heat found out since that time. Though for his own share he absolutely despairs of doing it, especially with any tolerable accuracy. “*Nobis ut ingenue fateamur*

* De Rer. Nat. i. 17.

“*crassiore*

“ crassiore ingenio donatis, & quibus non nisi ex-
 “ tremum vitæ spatium philosophari licuerit, mi-
 “ nimeque id molestiis, curisque vacuis—satis sit,
 “ si qui quantusque calor, quam molem, qua do-
 “ net dispositione intueri liceat. Et non exquisitè
 “ id quidem, distincteque, sed rudiore & confuso
 “ quodam modo, nec singulas caloris differentias,
 “ quæ innumeræ & roboris & copiæ sunt; nec
 “ materiæ conspissationes diversitatemque percipere
 “ potentibus, sed perpaucas utriusque & confuse
 “ illas, indistincteque.”

4. The nature of heat my Lord Verulam seems to have considered with more than ordinary attention; and gives his doctrine *de forma calidi* *, by way of example for managing philosophical subjects. And he found all the various heats not to differ in kind, but in their various intensities, durations, and other modifications and accidents †.

5. Sanctorio's Thermometer, given out to the world about that time, shewed all kinds of heat to have a similar effect in dilating the air; according to whose various expansions, the various intensities of those heats, of whatever kinds, were to be judged. The same was soon found to hold in

* Nov. Orig. II. 11, 12, 13, 18, 20.

† Ibid. p. 189, 196, 197, 200, 206, 208, 220, 252, 253, 254.

other fluids. And at length the firmest bodies themselves were, by undeniable proofs, found to swell by the application of heat of any sort. And from the various degrees of those dilatations alone we must take our measures of the various degrees of heat in bodies.

6. But where shall we begin to reckon the heat of bodies, or their greatest natural contractions, or the limits of heat and cold? Many things, when first proposed, look singular and odd, that, after laying aside prejudices, and by mature consideration become familiar and easy; and of the truth of which we come at length to be fully satisfied. We commonly conceive water, when once frozen and turned to ice, to have lost all heat, and so to have become absolutely cold; on which account many philosophers did too hastily begin their reckonings of heat from this point. But as various fluids, such as melted wax, melted tallow, oil, wine, brandy, &c. lose their fluidity at various degrees of heat, and after that may still grow colder and colder in their firm state; just so, after water begins to freeze, we know from many considerations it can become still much colder. That is, that in freezing water there is still a considerable quantity of heat; which by degrees grows less and less, till the cold becomes intolerable to us mortals; as in the dead of winter in the northern intemperate climates, where men cannot live without
more

more than ordinary shelter from the extreme severity of the weather. The air is oftentimes as much colder than water beginning to freeze, as such water is colder than our summer weather. We reckon it warm when the fluid in the Thermometer is raised to *gr.* 64, it is at *gr.* 32 when water freezes; and such colds have been often seen as to bring it down to *gr.* 0, the beginning of the scale, nearly the cold produced by a mixture of snow and salt.

7. Not as if this was the lowest degree of heat, or the greatest degree of cold that nature has ever been observed to produce on our earth, as Dr. Boerhaave * seems to think, and which he †, though I know not on whose testimony, tells us was the greatest cold that was observed in Iceland in the cold winter of 1708. The cold has been sometimes felt as great, or near as great as this in climates ordinarily temperate enough, and certainly much warmer than what we have reason to believe the bleak cold country of Iceland to be. At Germanopolis in Pennsylvania, only in the latitude 40°, *an.* 173½, the cold brought the mercury to *gr.* 5 ‡. In the famous winters 1708 and 1718, the cold at Paris was such, that it would have

* Chem. I. p. 162, 164, 166, 399, 400, 593.

† Ibid. p. 158, 166, 174.

‡ Aët. Berolin. Cont. IV. p. 130.

brought

brought down the mercury in our Thermometer to *gr.* 8 *. At Leyden, *an.* 172 $\frac{8}{9}$, it sunk to *gr.* 5 †, and at Utrecht it fell a division lower ‡. At London, in the winters 170 $\frac{8}{9}$ and 173 $\frac{0}{1}$, the cold was so violent as to bring the spirits almost down to the artificial cold of an ice and salt mixture ||: and *an.* 170 $\frac{8}{9}$ Mr. Roemer ** at Copenhagen, in the latitude of 55 deg. 43 min. found the mercury in his Thermometer to fall fully thus low, to wit, to *gr.* 0 ††.

8. But if we go farther north, though not so far as Iceland, we shall meet with much greater colds than any thing ever seen in these countries. In

* Mem. Acad. Sc. 1710, p. 186. 1717, p. 3.

† Boerh. Chem. I. p. 158.

‡ Musschenbr. in Phil. Transf. 425. p. 359.

|| Derham in Phil. Transf. Abr. IV. 2. p. 113. VI. 2. p. 50.

** Vide Boerh. Chem. I. p. 720.

†† Dr. Boerhaave speaks of this remarkable observation of Mr. Roemer as made at Dantzick. But I rather think it to have been made at Copenhagen: Dr. Derham (see Phil. Transf. Abr. IV. 2. p. 114.) had papers in his hands giving an account of this very frost *an.* 170 $\frac{8}{9}$ at Copenhagen, said to be taken from the observations of Mr. Roemer: and Mr. Roemer was a Dane, lived and bore high offices at Copenhagen, and died there *an.* 1710. However, in the main, this does not at all alter the story with respect to us.

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January 1732 the cold was so great at Upsal, that it brought the spirit of wine in one of the Royal Society's Thermometers down to *gr.* 124, which falls about a degree below *gr.* 0 in Fahrenheit's. And at Petersburg in the latitude only of $59^{\circ} 56'$, much the same with that of Upsal, *an.* 173 $\frac{2}{3}$, the cold was so excessive, that quicksilver was by it contracted $\frac{1}{80}$ part of the bulk it had in boiling water, or to fall to *gr.* 200 in De l'Isle's Thermometer*, which brings it to *—gr.* 28 in ours. We know in general, from the accounts of travellers, what intense colds rage in winter in many places of the world, and especially in or near the frigid zones in both hemispheres. But we have few observations on record exactly limited or reduced to a determined measure. However, in M. de Maupertius's† accurate journal, we find that the French academicians, who, in pursuit of knowledge, and to determine that important question of the figure of the earth, wintered *an.* 173 $\frac{6}{7}$ at the north polar circle, experienced a degree of cold much greater than any thing we find measured and recorded by others. At Torneao in the latitude of $65^{\circ} 51'$, even on this side that circle, their spirit of wine Thermometers came at length to be frozen, and a mercurial one constructed in Mr. de Reaumur's way sunk to *gr.* 37 under his freezing point, which

* Phil. Transf. 441. p. 222.

† Fig. de la Terre, p. 58.

would

would have been about 33 divisions below *gr.* 0 in Fahrenheit's. When that intense cold was suddenly admitted into their warm rooms, their bodies could hardly suffer it, their breasts were as if they had been rent, and the moisture of the air was in an instant converted into whirls of snow. This is a degree of cold as much below the cold of freezing water, as that is below the ordinary heat of our skin: for it is $33+32=65$ divisions below the freezing point at *gr.* 32, and the heat of the human skin at *gr.* 97 is just so much above it. $51+32=83$

9. To find a cold so far below that of freezing water is certainly very surprising: and yet a greater than this almost inconceivable degree of cold may by art be procured in less rigid climes: for we can contrive a method of cooling much beyond the ordinary tendency and course of nature. Many things when mixed together immediately become vastly colder than formerly when they subsisted distinct from one another. Spirit of nitre poured on beaten ice or snow brings on a very intense cold. These, when their temperature and that of the air was but about *gr.* 32 above *gr.* 0, the ingenious and industrious Fahrenheit by an artful management cooled so much that the mercury in the Thermometer sunk to—*gr.* 40, or 72 divisions below the freezing point*; a degree of cold great- $= 32 \text{ gr.}$
in Reaumur's

* See Boerh. Chem. I. p. 162. Musschenbr. Tent. Exp. Acad. Cim. Ad. p. 174.

er than what was felt in the neighbourhood of the polar circle: and this is as much below the cold of freezing water, as that is below the heat of our hottest animals, or of men in fevers. No body would have thought, *a priori*, that freezing ice was capable of such an additional quantity of cold.

10. Nor are we absolutely sure that yet in these circumstances all heat was destroyed. Even in that horrid and most bitter cold at Torneao, which the French mathematicians mention with so much horror, we are sure there was; and likewise even in that excessively cold mixture of Fahrenheit there might still be, and probably there was, some degree of heat. What if there was a concurrence of such frigorific circumstances? If Fahrenheit's experiment were to be made in the frigid zone, what would be the effect, and what a dreadful cold might by that means be produced? * So then, for want
of

* When I was lately at Paris there were put into my hands some very extraordinary observations, communicated to the Royal Academy of Sciences by Mr. De l'Isle at Pétersburgh, of excessive colds at Kirenga in Siberia, a place not very far north neither, lying in the latitude only of 58° , $10'$. There in the winter 1737 the mercury once fell to *gr.* 275 in De l'Isle's Thermometer, which by calculation I find should be *gr.* 118 below the beginning of the scale in ours. This, if there be no mistake in the affair, is the most astonishing

of knowing the ultimate limits of heat and cold we cannot determine the geometrical proportion of the real or absolute quantity of the heat of one body to that of another. All we should pretend to is to assign their arithmetical differences; which is a very useful work, and is to be done by a great number of accurate observations on the various expansions of bodies by the various degrees of heat: these expansions always corresponding in some measure to the quantities of fire or heat applied. So that we shall in the ordinary way speak of these things as proportional to one another. Though I would not be understood positively to affirm that they both go on always precisely in the same ratio: or that the expansions were always exactly in a ratio neither greater nor less than the simple ratio of the quantities of the applied heats.

11. When people * tell us that red-hot iron is 3 or 4 times hotter than boiling water, and that boiling water is about 3 times hotter than the heat of our skin, or of the summer heat, they generally reckon the lowest limits of heat to be where wa-

ing thing I ever met with of the kind. It supposes a cold 150 degrees below the freezing point, almost an incredible thing: a cold as much below the cold of freezing water, as that is below the heat of boiling spirit of wine.

* Vid. Newton Princip. p. 508. & in Phil. Trans. Abr. IV. 2. p. 2, 3. Pitcarn. El. Med. II. 1. § 26.

ter begins to freeze; and thence found the additional dilatations of quicksilver, or of lint-feed oil (or of any other fluid that may be used in such Thermometers) to be in these proportions; though the absolute or real quantities of heat in these bodies do not differ so much by far, as they by that mistaken way of reckoning computed. Thus boiling water indeed raises the mercury in the Thermometer to *gr.* 212, that is 180 divisions above *gr.* 32, the heat of freezing water, while the heat of the human body is but about *gr.* 97 or 98, only 65 or 66 degrees above the freezing point. And therefore the dilatation acquired from that point by the heat of boiling water, to the dilatation acquired by the heat of our body, is in the proportion of 180 to 66, or 2'73 to 1; which is something less than triple. But the real heats of these things are still less differing from one another. Suppose for example Fahrenheit's ice and spirit of nitre mixture of —*gr.* 40 had been at the lowest boundary of heat, (though there is great reason to think that to be still much lower), and then we shall find that the heat of boiling water should be to that of our bodies only as $212 + 40$ to $98 + 40$, or 252 to 138, or 1'83 to 1: that is, not so much as double. Who, before such a particular examination, would have thought the burning destroying heat of boiling water not to be altogether twice so great as the soft temperate heat of a man in health? And upon the former reckoning of red-hot iron,

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its heat should be to the heat of our skin instead of 10 or 12 to 1, only as about 700 to 138, or 5 to 1. So that by thus bringing down the limits of heat at least 72 degrees below the ordinary reckoning, we find its various characters as *frost, temperate, warm, rapid, boiling water, red-hot iron, &c.* whatever their arithmetical distances come out to be, are not so different in their real proportions to one another, as is commonly supposed. Though, on the other hand, I would not affirm them to differ so little as Mr. Amontons * reckoned. In whose scheme the air should entirely lose its elasticity, when it is perfectly devoid of heat (of which however I think we are not well assured) and then in such a total absence of heat, were it possible to procure it, reducing his numbers to ours, the mercury in Fahrenheit's Thermometer should fall 431 divisions below the cold of freezing water, or to —gr. 400 *p. q.* a degree of cold as much below the heat of the human body, as that is below the burning heat of oil or mercury boiling. But as we are not quite certain about this lowest degree of heat, we must, as I just now said, content ourselves to observe the arithmetical differences of the various degrees of heat in bodies, though we should not be able to determine their real or absolute proportions, until we shall acquire a farther insight into the na-

* Hist. Acad. Scienc. 1702. p. 8. Mem. 1703, p. 63, 64, 238.

ture of things than we have at present. It is good to know the narrow limits of our faculties, and modestly to confess the bounds of our knowledge; ever keeping in mind that excellent axiom of the Lord Verulam *, “Homo naturæ minister & interpreter tantum facit & intelligit quantum de ordine naturæ opere vel mente observaverit: nec amplius scit aut potest.” It must be by this road of experience and observation, pointed out to us by the great Lord Chancellor, if ever we arrive at a higher degree of knowledge than what we at present enjoy.

II. *Of the heat of the Air.*

12. In our search † after the lowest degree of heat, though we could not arrive at it, we found however in some seasons and parts of the world excessively great, and as it were monstrous degrees of cold; and which the inhabitants of the earth can scarcely bear; they are intolerable both to animals and vegetables. The quarters of the earth, within the polar circles, were thought by the ancients to be uninhabited by reason of the excess of cold prevailing there, ἀνοικητοὶ διὰ ψυχρῶς ‡.

* Nov. Org. I. 1.

† § 6—10.

‡ Diog. Laert. VII. 156.

*Frigus iners illic habitat, pallorque, tremorque, ac
jejuna fames.*

“Quia,” (says Macrobius *), “torpor ille glacialis
“nec animali nec frugi vitam ministrat.”

*Pigris ubi nulla campis
Arbor æstiva recreatur aura †.*

And indeed these bleak wild parts of the world, though not quite waste, are but thinly peopled. And no wonder. There, and in other disadvantageously situated places, where such intolerable colds chiefly rage, men must shelter themselves in clothes and houses. The beasts of these countries are many of them provided by nature with a thick warm covering; or must retire the best way they can from the killing colds of winter, which however often overtake them. The very vegetables themselves would die were they not commonly in these seasons buried under snow to such great depths, as the extreme severity of the cold cannot reach. In bitter black frosts we often experience the fatal effects of the want of this shelter and protection, even in the humbler tribe of vegetables. And though there be plenty of snow, the trees and higher shrubs, that are not covered by it, are great sufferers in the extraordinary severe winters,

* In Somn. Scip. II. 5. p. m. 110.

† Horat. Carm. I. 22.

even in the more happy and otherwise more temperate countries. As has been often seen, and in our own days was experienced through all Europe in the years 170⁸, 172⁸, &c.*; when the Thermometers pointed these low degrees of heat we had formerly occasion to mention.

13. But these and such like excessive colds are in these parts of the world justly looked on as a sort of irregularities, and out of the ordinary course of things. In these temperate climates of ours the mercury seldom falls under *gr.* 16. And and even then it is extraordinary cold. And indeed we are apt to reckon it very cold at *gr.* 24. By the very construction of the Thermometer it is slight frost at *gr.* 32: and continues coldish to *gr.* 40, and a little above it.

14. The middle temperature of our atmosphere in these countries is about *gr.* 48, when we cannot call the weather either hot or cold; and is, as it were, a medium of all the seasons, coinciding pretty nearly with the middle vernal and autumnal heat, as observed in England by Sir Isaac Newton, and Dr. Hales, and by Mr. Crucquius in Holland. The French raise this middle temperature a little higher. They reckon it equal to the heat of the

* Phil. Transf. Abr. IV. 2. p. 120, &c. Mem. Ac. Sc. 1710, p. 186. Hales Veg. Stat. p. 74. &c.

cave of their Royal Observatory: and this, both by observation and a calcul, comparing it with Mr. Amontons's Thermometer, I find to be at *gr.* 53 in Roemer's. And indeed this middle temperature of the air we may easily conceive to be very different in different countries. It is to be reckoned as a relative sort of thing, varying according to the various climates and constitutions of bodies in them, which by use come to be fitted to different temperatures. In the cold countries the air will be found agreeable enough to the inhabitants while it is betwixt *gr.* 40 and 50. In these middle climates in our neighbourhood, and not too far south of us, we are best pleased with the heat of the air from a little under *gr.* 50 to *gr.* 60; while in the hot countries they have their air generally, and can well bear it, something both above and below *gr.* 70.

15. When the heats in the respective climates are either much higher or much lower than these, both men and beasts are fain to provide against them the best way they can. In the cold they shelter themselves in houses, dens, holes of the earth, &c.; and the human kind beside that provide for themselves clothes and fires, to protect them from the severity of the weather. In the heats we lay aside our clothes, or take to the lightest sort; we shun the mid-day air, and retire to the coolest places; for in the day-time it is generally warmer
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in the open air than in houses. And some of the bird kind chuse to shift their country in the different seasons of the year, finding always by this means a temperate enough climate for their constitution.

16. But the same degree of heat is not equally fitted to all animals. Some kinds naturally chuse a cold country, while others indulge themselves in a warmer climate, and find that better fitted to their constitutions. And so too, though with Dr. Hales* we should deem the most genial heat for the generality of plants to be from *gr.* 53 to *gr.* 69, the various classes of vegetables have their various temperatures of the air in which they can thrive best. Every body knows that some plants can live only in the cold countries, being burnt up if transplanted to a more southern soil; while the natives of the warm countries are chilled to death with us, if not very carefully and artfully protected from the injuries of our severer weather. This piece of culture, proposed long since by the great Lord Verulam*, has been, in our time, carried to a surprising height. With the help of Thermometers we can so adjust our stoves, green-houses, and hot-beds, as to imitate the temperature of any

* Veg. Stat. p. 59.

† Nov. Org. II. 35. p. 254. 50. p. 348. Nat. Hist. 401, 405, 412, 856.

climate we please; and to support and keep in strong life and vigour the plants which nature has given only to the warmer countries. According to the observations of the London gardeners, as marked on Mr. Fowler's Thermometer reduced to ours, I find the kindly degrees of heat for some of the most curious foreign plants to be these. For the myrtles *gr.* 44; oranges *gr.* 47; ficoides *gr.* 50; Indian fig *gr.* 53½; aloe *gr.* 57; cereus *gr.* 60; euphorbium *gr.* 63; piamento *gr.* 66; ananas *gr.* 70; melon thistle *gr.* 73.

Not as if these plants could bear only such and such precise points of heat. They, like other vegetables and animals, have a considerable latitude that way; and can suffer the air both colder and warmer than these degrees, which, however, are reckoned most adapted to their nature. Though I suspect some of them are marked too low, being far under the ordinary and most kindly heats in those countries where such plants naturally grow. But there indeed they have a freer and opener air; where consequently they can bear greater heats than in our smothered and pent up stoves and green-houses.

17. Even we in this country do not reckon the air warm till it arrives at about *gr.* 64. It is to us very warm and sultry at *gr.* 80. And Dr. Boerhaave* thought it naturally scarce ever exceeded

* Chem. I. 156. p. 192, 207, 213, 274, 278, 553.

gr. 80 or 90 at most, reckoning such hot air would soon be destructive of the life of animals. But sometimes, even in these temperate climates, the weather has been found much hotter. I find observations recorded of the weather at Utrecht, Paris, Padua, &c. having sometimes been observed so hot, as that the mercury in our Thermometer would have been raised to above 90 degrees. In Pennsylvania, no intemperate climate neither, the heat in summer 1732 came once to be *gr.* 96 or 97*. And Mr. de Reaumur† promises us observations of heats that people were obliged to suffer, though they reached to *gr.* 38 in his Thermometer, or near *gr.* 104 in ours. Even in the more northerly countries, where the sun has less influence, we can by art contrive a method of giving undeniable evidence of animals being able to bear a still greater warmth of air. In the outer bagnio at Edinburgh the heat used to raise our Thermometer to *gr.* 90, where, though at your first entry it seemed indeed a great deal too hot, and something disagreeable, yet soon it became so very tolerable, that one might without any uneasiness have continued in it as long as he had thought fit. Yea, one could stay some hours in the inner bagnio, though the heat there used to be about *gr.* 100.

* Act Berolin. Cont. IV. p. 131.

† Mem. Acad. des Sc. 1736, p. 486, 489.

18. From all this we see how finely every thing has been contrived and adjusted by the great and wise Author of nature, and what a vast latitude of the heat and cold of the air the animal body, especially that of man, can bear, from the sultry burning heats, as hot or hotter than his blood, down to the chilling colds a good deal below that of freezing water; though indeed it is about some intermediate distance betwixt them, where the heat is most agreeable, or best tempered to the bodies of animals and vegetables.

19. And it was only the middle climates or temperate zones that many of the ancients imagined were habitable by us mortals.

*Mortalibus agris
Munere concessæ divûm*.*

“In his,” says Macrobius†, “tantum vitales auras natura dedit incolis carpere.” And accordingly much was said long ago of the intolerable heat of the torrid zone, as by that means unin-

* Virgil Georg. I. 237.

† In Somn. Scip. II. 5. p. m. 110.

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habitable by men, ἀνοικητος ὑπο καυμάτων *: whence Ovid †, speaking of the zones of the earth, says,

Quarum quæ media est non est habitabilis æstu.

Virgil ‡ says the same thing in a more pompous manner.

*Quarum una corusco
Semper solē rubens, & torrida semper ab igni.*

And Horace ||, with his ordinary poetical imagery, supposes himself placed,

*Sub curru nimium propinqui
Solis, in terra domibus negata.*

But Tibullus ** expresses the position of this part of the earth, and its consequences, more fully than any of them, in these lines :

*Media est Phœbi semper subjecta calori,
Non ergo pressō tellus consurgit aratro :
Nec frugem segetes præbent, nec pabula terræ.
Non illic colit arva Deus, Bacchusve, Ceresve,
Nulla nec exustus habitant animalia parteis.*

* Stoics in Diog. Laërt. VII. 156.

† Metamorph. I. 49.

‡ Georg. I. 234.

|| Carm. I. 22.

** Eleg. IV. 1. in Messal.

But this opinion was, I humbly think, among their vulgar errors, received chiefly by the people, and taken up by the poets as a beautiful fiction, which we cannot well suppose they really believed. The wiser sort, though they sometimes spoke * like other folks, knew better things. Their historians and geographers have recorded tolerable good accounts of some of the middle parts of the earth within the tropics. *Æthiopia*, *Arabia Felix*, the sea-coasts of *India*, the isle of *Taprobana*, *Ophir*, *Tarshish*, &c. were heard of even by the common people, all well inhabited, though lying within the torrid zone: and the travels of the ancients, as well as ours, shewed these countries not to be disagreeable or inconvenient for human life. Nay, the heats there are by no means so excessive as we are all, before trial, ready to imagine and expect. The Jesuit missionaries to the East Indies † took particular notice of this: and their capacity of observing such things, and their fidelity in relating them, no body will call in question. And lately the curious Mr. Cossigny's thermometrical observations ‡ shew, that the ordinary heats, even under

* See Cicer. *Somn. Scip.* 6. Macrobi. in *Somn. Scip.* II. 5, 6, 7. Diog. Laërt. VII. 156.

† See Du Hamel *Hist. Ac. Sc.* p. 272, 273. *Hist. Acad. Sc.* 1666–1698, II. p. 111, 112. *Mem. Acad. Sc.* 1666–1698. VII. p. 835.

‡ *Mem. Acad. Sc.* 1733, p. 580, &c. 1734, p. 759 &c.

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the line, and near it, are not greater than what have been in some extraordinary seasons observed at Paris, and other places, that every body knows to be temperate enough.

20. It is true all these observations were made in islands, and on or near the sea-coasts, which we may presume to be somewhat cooler than the inland countries*. But still we are well assured, that even these can easily enough be borne with. And all these parts of the world have that advantage of the weather there being much more uniform than with us. Their air suffers much smaller changes both in the incumbent weight of the atmosphere pressing it, and in the degrees of heat wherewith it is warmed. Dr. Halley† tells us, that he, as well as others‡ who have sojourned within the tropics, found very little variation in the height of the mercury in the Barometer, which we know to be 2 or 3 inches in the more northern regions: and we are all but too sensible, and we had but just now occasion to take notice of the vast changes of the temperature of the air in this quarter of the world, which through the whole year is commonly found so equable in the torrid zone. At Siam, within the lat. 15 deg. from the greatest winter cold to their

* See Mem. Acad. Sc. 1666—1698, VII. p. 837.

† Phil. Transf. Abr. II. p. 18, 20, 22.

‡ Vid. Mem. Acad. Sc. 1666—1698, VII. p. 840.

highest

highest summer heat the Jesuits experienced a difference only of 26 degrees in Hubin's Thermometer *, the extent of which I believe differs not much from the like number in ours. † In Batavia, though a very hot place, and in *gr.* 6 south lat. the excess of heat above the greatest cold was still something less: and in Malacca ‡, but 2 deg. of latitude from the equator, the weather is very temperate, and so equable, that for 7 months together the spirit in the Thermometer was never under *gr.* 60, nor above *gr.* 70. || In the isle of Bourbon, in the latitude about 22 degrees the difference between the highest and lowest afternoon heat in a twelvemonths time was but 8 divisions in De Reaumur's Thermometer, which make up scarcely 15 in ours: and the ordinary difference of the night and day heat in these climates was found a small matter compared to what we have it commonly with us **. And as the torrid zone excels our countries hereabout in a more equable degree of heat and weight of the atmosphere, so we are more happily appointed that way than in Sweden

* Du Hamel Hist. Ac. Sc. p. 272.

† Ibid. p. 273.

‡ Ibid.

|| Mem. Ac. Sc. 1734, p. 762

** Ib. 1666—1698, VII. p. 837. 1733, p. 537.

and Lapland, and these other northern regions*. From whence it would seem a general rule, that, *ceteris paribus*, the greatness of the vicissitudes of the air increases with the latitudes of the places as you go from the equator.

III. *Of the comparative Heats of the Sun, Earth, Planets and Comets.*

21. We have hitherto considered the air, though influenced by the sun, as shaded from its immediate burning operation. For in the open air, exposed to its direct beams shining with greatest advantage, the heat is found much higher than any thing we have hitherto been speaking of. And an examination of this will naturally lead us into the consideration of the heat of the sun, the inexhaustible magazine of light and fire, the great furnace of our system; and to consider its influence on bodies placed at different distances from it.

Sir Isaac Newton† comparing the sun's influences on the earth and Mercury, which by its vicinity to the sun should have seven times more heat from him than we, reckons the solar heat in that planet sufficient to boil our water, and quickly carry it all off in vapour; having found by a Ther-

* See Phil. Transf. Ab. II. p. 22. Maupert. Fig. de la Terre, p. 72.

† Princip. III. Prop. 8. Cor. 4. p. 406.

mometer that water was boiled by a heat seven times greater than the heat of the summer sun; which then in this reckoning should be only at gr. $(32 + \frac{1}{7} \times 10 =) 58$. But this expression of *the heat of the summer's sun* must be greatly modified to be consistent with the true state of things. He must not only have meant it solely of the degrees above freezing water, (as was then the fashion of thinking and speaking), but also instead of *summer sun*, he must really have understood *the ordinary shaded air in summer*, or *the summer heat of the air*, as Dr. Pitcairn* (who I suppose had his observations of this matter from Sir Isaac) expresses it. Though I must confess that this could be but very improperly compared to the solar heat at Mercury; except we suppose that he likewise meant the heat in the shade at Mercury too. And farther it will be reckoned an incongruous way of judging of the heat of the sun by taking our measures from its impaired action on the air screened from its direct rays.

22. The direct summer mid-day heat is vastly greater than such a low allowance; it having been found, even in these northern climates, able to dilate the fluid in the Thermometer much more than $\frac{1}{7}$ of the dilatation caused by boiling water. Dr. Boerhaave is the lowest in his reckoning of any that pretended to take an estimate of it, saying †,

* Elem. Med. II. 1. § 26.

† Chem. I. p. 213.

that the greatest heat ever produced by the direct action of the sun in our air, or other bodies heated by it, seldom reaches to *gr.* 84. And at another time *, though he raises it a little higher, seeming to allow that the great dog-day heat may come to *gr.* 90, he thinks it can scarce ever go beyond these bounds, or arrive at the heat of the human-body. But Sir Isaac himself in another place of his book † expressly tells us, that he found the heat of boiling water only about three times greater than the heat communicated to dry earth by the summer sun. And so too Dr. Musschenbroek ‡ reckoned it probable that the heat of boiling water is three times greater than the greatest heat communicated to bodies by the summer sun in his country; by these reckonings this heat amounting to *gr.* $(32 + \frac{18}{3} =) 92$. Nay even in Italy Borelli || and Malpighi § found this heat of the sun in midsummer only equal to the heat of the *viscera* of hot animals, I suppose about *gr.* 102; not a great deal higher than what Newton and Musschenbroek reckoned.

23. All this may, I believe, commonly be just and true enough. But it is not so universal, either

* Chem. I. p. 156.

† Princip. p. 508.

‡ Tent. Exp. Ac. Cim. Add. II. p. 22.

|| De Mot. Anim. II. Prop. 96. 221.

§ Op. Post. p. 30.

as to times or places, as one would be ready to expect from such ways of speaking. The Lord Verulam * long ago took notice, that the highest degree of animal heat scarce ever attained to the heat of the sun's rays in the scorching climates and seasons. And even in this cold country at sometimes I have experienced greater heats of the sun; and so might any body else that would attend to it have done. Many have had occasion to observe how vastly metals are heated in the sun. Sir Isaac Newton † takes particular notice of this; I have in this place in the latitude of 56 deg. 20 min. seen a piece of iron heated so much that I could not hold it in my hand above a few seconds of time; and therefore it was much hotter than my blood. And I have oftener than once seen dry earth heated to above *gr.* 120. And Dr. Hales ‡ found a very hot sunshine heat *an.* 1727 still greater, *viz.* at about *gr.* 140. And Dr. Musschenbroek § in his later trials once experienced it so high as *gr.* 150. So that reckoning from the freezing point at *gr.* 32 in the common way, these strong sun-shine heats were much greater than $\frac{1}{3}$, or even the half of the heat of boiling water. And in the more southern countries it has been found still greater at some particu-

* Nov. Org. II. 192.

† Princip. p. 420.

‡ Veg. Stat. p. 59.

§ Ess. Phys. § 974.

lar times. Much has been said of the scorching and intolerable heat of the sun in the torrid zone *. We have many strange stories of extraordinary summer heats, as great tracts of land, houses, &c. set on fire, stones heated so as to melt lead, &c. These indeed seem extravagant. But the German annals † preserve the memory of an excessively hot summer *an.* 1230, when they roasted their eggs in the sand heated by the sun. And I have been told that in Egypt, by no means the hottest country in the world, they can often on the tops of their houses roast their eggs at the sun. And to harden the white of an egg I find the heat of about *gr.* 156 to be necessary. In the year 1705 the summer was very warm ‡. At Montpelier one day the sun was so very hot as to raise the quicksilver in M. Amontons's Thermometer to the mark of boiling water itself, which is our *gr.* 212.

24. But after all, these great heats will be reckoned irregularities, and out of the ordinary course of the seasons; and what may be produced by the sun only in certain times and circumstances. For in our temperate climates the most common and ordinary noontide heat of the summer sun may be found to raise the quicksilver in the Thermometer but about 60 or 70 divisions above the freezing

* See Boyle Hist. of the Air. Abr. 3. p. 55.

† See Lipshii Epist. ad. Belg. II. 91.

‡ Mem Sc. 1706. p. 15.

point at gr. 32; and the afternoon heat in the shade about the half of that. So the ordinary summer sun heat with us does nearly coincide with the heat of our body; which was found * to be somewhat more than the half of the heat of boiling water, supposing Fahrenheit's cold mixture to be the very lowest degree of heat; and so too boiling water, instead of seven times, is but between two and three times hotter than our common shaded warm air at midsummer.

25. However this solar heat is still much greater in the warmer countries. And if we pretend to make any comparison of such things, it is our equatoreal heat we should reckon as the sun's true heat at our orbit, and what we should collate with its influence on the other planets, or its heat at their respective distances.

26. But what, on the other side, will lower or bring down this solar heat very much is this consideration, that the heat communicated by the sun to bodies on this earth depends much upon other circumstances beside the direct force of its rays. These must be modified by our atmosphere, and variously reflected and combined by the action of the surface of the earth itself, to produce any notable effects of heat. For were it not for these ad-

* See § 11.

ditional circumstances, I question much if the naked heat of the sun would to us be very sensible. We find all our great heats to be in those places that ly low, and have a great height of atmosphere above them, and surrounded by eminences and rising grounds. * If you ascend on high to the tops of the very elevated mountains, you are chilled with cold, and you find everlasting snows, that after thousands of summers have scarce ever been thawed, though every day exposed to the direct rays of the sun; which in some countries are darted perpendicularly on them. We hear that the French virtuosi, who by order of the king went to America to make their observations on the figure of the earth, &c. found on the very high hills there just under the line such excessive colds, that it is suspected they were as intense as what were observed in Lapland itself. So necessary it seems is a long and direct passage through all or the greatest part of the depth of our atmosphere, or the assistance of its pressure, or the reflections of rays from the earth's own surface, to invigorate these rays, and to give them strength for warming terrestrial bodies. To which too the particular sulphureous

* See Liv. Hist. XXI. 31. Senec. Nat. Qu. IV. 11. Bacon. Nov. Org. II. 12. p. 169. Borell. de Incend. Actu. p. 7, 50. Boyle Exp. on Cold, Abr. I. p. 639, 658. Hist. of the Air, Abr. III. p. 53, 54, 55. Boerh. Chem. I. p. 172, 185, 476.

nature of the low parts of the atmosphere may not a little contribute.

27. So then there are other circumstances beside the vicinity to the sun that may influence and determine its heating virtue. Thus reckoning the force of heat to be *ceteris paribus* as the density of its rays, or reciprocally as the squares of its distances from the central fire, the planet Mercury is so much nearer the sun than the earth, that its heat from him, by calculation of their respective distances, comes out seven times greater than ours*. And so would an inhabitant there feel it, if they have the surface of Mercury disposed like that of our earth, and an atmosphere hanging over their heads so like ours, as to qualify the sun's rays in the same manner. But if they have little or no atmosphere (which seems to be the case of our moon) or an atmosphere so disposed as only to affect the rays of heat in the manner that the upper parts of our atmosphere do, then it is possible the solar heat at Mercury may be as tolerable as on our earth. And, on the other hand, though we commonly reckon † that our waters should be frozen up, and we ourselves be chilled to death in the far-off planets Jupiter and Saturn, they may, for aught we know, have their atmospheres and

* See Newton Princip. III. Prop. 8. Cor. 4. p. 406.

† See Newton, *ibid.* p. 405.

surfaces so constructed, as to render the solar heat comfortable enough even for animals and vegetables of the like nature and passions with our own.

28. But the case of the comets is vastly different from any thing we can propose of the other planets. Their orbits are so excentrical, that they must be very variously affected by the sun in the different parts of their course. But yet their atmospheres and bodies may be of such a nature as that they shall not be heated to such a vast and almost inconceivable degree as is sometimes supposed. In the Newtonian system of comets * the distance from the sun of that famous one which appeared *an.* 1680 † in its perihelion was, by calculation, to the mean distance of the earth in the ratio of about 6 to 1000: so that its heat in that

* Newton Princip. III. &c. p. 508.

† It is supposed by many to have been this very comet that appeared *an.* 1106, *an.* 531, *an.* A. C. 44, &c. See Newton Princip. p. 501. Halley Synops. of Comet. in Gregor. Astron. in English. p. 901, 902, 903. Whiston New Theor. of the earth. p. 187, 191, &c. But M. Cassini represents the comet, or comets, *an.* 1680 in a very different manner from these authors; nor does he make that great blazing star that alarmed all Europe, to have been so very excentrical, or to have come so very near the body of the sun as they do. See Mem. Acad. Sc. 1731, p. 464—468.

part of its period Sir Isaac determines should have been to our summer heat reciprocally as the squares of these numbers, that is, as 1,000,000 to 36, or 28,000 to 1. And therefore, seeing the heat of boiling water is three times greater than what dry earth acquires from the summer sun, and the heat of red-hot iron three or four times greater than the heat of boiling water, he computes the heat of the comet should have been 2000 times greater than the heat of red-hot iron. A prodigious and terrible heat, even with all the abatements we have been proposing; and which I conceive no body can take on, that is not immensely denser and more fixed than any terrestrial body we know. For, from the example of common water, &c. * it would seem that all bodies are, according to their various degrees of density and fixedness, susceptible just of such and such particular degrees of heat, beyond which, whatever fire be applied to them, they cannot be raised; though by a heat exceeding these their parts are all dissipated, are rendered volatile, and fly off in vapour. And yet I cannot but take notice, that all this calculation is upon the supposition that the comet was in its own nature like our earth, and involved in an atmosphere like ours: otherwise that method of reasoning and analogy cannot take place, nor would the sun's heat affect it in that way.

* See Boerh. Chem. I. p. 265, 248.

29. But I say again, under another view and consideration, what if the real solar heat, both in itself and what it can communicate to us and other planetary bodies, (while it is not concentrated by burning glasses, or strengthened by other assistances), be vastly less than what is commonly reckoned? All the natural heat we meet with here on the earth we are ready to ascribe to the action of the sun, which perhaps has but a small share in it, overlooking a source of heat, which, though often spoke of by the theorists of the earth*, is seldom considered in that advantageous light I would chuse to take it. We formerly took notice of what a great stock of heat all terrestrial bodies are possessed, even in the coldest winters. Every body has felt or heard that the temperature of the air in mines and other places deep under ground is warm, or at least very tolerable. And we know, from the nicest observations, that in the cave of the Observatory at Paris, only about ninety feet under ground, the heat keeps the Thermometer at *gr.* 53, and that without any assistance from the sun; it being never sensibly increased by the most

* See Empedocles in Plutarch. de Prim. Frigid. p. m. 507. Des Cartes Princip. Phil. IV. 3. Burnet Theor. of the Earth, III. 6. Woodward Ess. Nat. Hist. of the Earth, p. 135—162, 220—225. Nat. Hist. &c. Illust. &c. Introd. p. 136, 140—143, 149—152. Whiston New Theor. &c. p. 78, 231, 334, 447. Gassend. Epicur. Philosoph. I. p. 546, &c.

scorching

scorching seasons beyond its heat in the most severe winters that have been felt there. And the same constant and unalterable degree of heat was observed by Mr. Boyle * in a cave cut deep into the earth. † And great and even troublesome heats are said to be observed at greater depths, and increasing in proportion to these depths; though I could wish these heats had been more regularly measured and ascertained than what I can find they have yet been. So that it would seem the body of the earth has a very great proper internal heat, independent on the sun, and very much beyond what he, without the intervention of our atmosphere could communicate to it; so great as, within 90 feet of its surface, to raise the Thermometer 93 divisions above Fahrenheit's cold mixture, or 453 divisions above what Mr. Amontons reckoned the lowest degree of heat. This heat of the earth at its surface is something less, and beyond that its force decreases indeed very fast, so as to leave the air at a small height above it a good deal colder; and which we find on the very high hills to be excessively cold, and not to be much warmed by the additional heat of the sun's direct rays, if they be but little altered by the earth's surface and atmosphere.

* Mem. Hist. of the Air, Abr. III. p. 54.

† See Boyle Exp. on Cold, Abr. I. p. 641, 700, 701, 702. Boerh. Chem. I. p. 479. Woodw. Eff. Nat. Hist. of the Earth, p. 136.

30. It is not our business so much as to attempt the explaining the cause and consequences of this innate heat. That is the work of those who have fire and fancy enough to build worlds, or theories of worlds. I will only beg leave to observe, after the Lord Verulam*, Mr. Boyle†, and others, that the various temperatures of the air in different places of the earth, do by no means correspond to what should be the result of their position to the sun. To omit variations that are small and of less moment, I shall only take notice of the much greater cold in all the southern hemisphere than in the similarly situated places of the northern one; and of the greater colds in the North American countries, than in those of the same latitude on the European side of the globe. All which argues a copious fund of some other more potent cause of heat than the regular actions of the sun; and that that cause, inherent in the earth itself, is stronger in some regions of it than in others, though every where considerable and of great force.

31. The ingenious Dr. Halley‡ has a very bold hypothesis, which, beside other purposes, he em-

* Seq. Chart. p. 102

† Exp. on Cold, Abr. I. p. 657—660, 670. Mem. Hist. of the Air, Abr. III. p. 52.

‡ Phil. Trans. Abr. VI. 2. p. 41.

plays in accounting for this anomaly of these warm and cold quarters of the world. He imagines that once the earth moved round an axis very different from the present, and so that " the extreme cold
 " felt in some places, as in the north-west of Ame-
 " rica, about Hudson's Bay, may be occasioned
 " by those parts of the world having once been
 " much more northerly, or nearer the pole than
 " now they are, whereby there are immense quan-
 " tities of ice yet unthawed in those parts, which
 " chill the air to that degree, that the sun's
 " warmth seems hardly to be felt there;" which, after some thousands of years, have not yet, it seems, acquired a sufficient heat: while by a parity of reason we may from the same principles infer, that the extraordinary soft temperature of some other places may be owing to their having originally been under or near the primitive equator, whereby they had acquired a heat which is not yet quite dissipated.

But, besides that in such an hypothesis those places should constantly be coming nearer and nearer to a regularity and uniformity with other countries alike in latitude and other ordinary circumstances, the heat of bodies is so transient and fleeting, and the influence of the sun reaches so little way into the body of the earth, that I cannot conceive, even allowing the supposition of a changed axis, that the various degrees of the original primitive heats of the earth, communicated to it
 by.

by the sun, could possibly have been so permanent and lasting, as to shew themselves so effectually at this day: so that, as I said before, such a variety of heats must rather be owing to an internal cause operating more intensely in some regions than in others, and every where much more strongly than any influence the sun can give them.

32. And if this be the case of the rest of the planets as well as of our earth, their heats, or the heats of bodies on their surfaces, will not solely nor even chiefly depend on the action of the sun on them, (as the philosophical astronomers * seem by their manner of speaking to suppose), these proper innate heats having a greater share in them than that external addition. Though this, however, may be necessary to support and preserve those entire; seeing all warm bodies, that are not endowed with a vital principle of heat, are, of their own accord, liable to a constant waste and decay of this quality.

33. But whatever be in this, from these considerations, the heat of the sun, and the heats of bodies approaching near him, will not be found so

* See Newton Princip. p. 405, 406, 508, 509. Gregor. Elem. Astron. VI. Prop. 2, 3, 4, 5, 6. Whist. Præl. Math. p. 327, 328. New Theor. &c. p. 53, 78. Hugon. Cosmotheor. II. p. 694, &c. Derham Physico-theol. p. 171—178.

exorbitant as we have been accustomed to reckon them *. The proper or innate heats of the planets may be such, that the inner ones are in no hazard of being too much scorched, nor the far-off ones of having too small an allowance of the benign influence of the sun's heat; and that, especially by the help of peculiarly disposed atmospheres concurring that way, there may be a great uniformity, in this respect, of the whole solar system. Yea, even the comets themselves may, for any thing we know, be so constructed as to suffer no great inconvenience by their great excentricities, and very variable distances from the central fire.

34. But where am I going? I find myself treading on slippery and enchanted ground. I fear I may be thought just now to have got too far into the bewitching and airy regions of fancy and conjecture, where I would not chuse to dwell. However, at present having a delightful prospect in view, I would fain be indulged in one question before I quit this place. What if, abstracting from the other solar influences of light, gravitation, &c. the great force and virtue of its additional heat on the bodies of planets (which is wisely contrived to be very variable and shifting) is to produce frequent changes, on the otherwise equable heats of their surfaces, for very great and noble purposes

* See Newton, *ibid.* p. 508. Whiston, *ibid.*

in the life and support of their inhabitants? as at least we know to be the state of our globe, which, by the vicissitudes of night and day, and winter and summer, and spring and autumn, beside other ends thereby accomplished, preserves in action and life both vegetables and animals; all which would stagnate and die in a state of heat always perfectly the same: for all natural organic bodies, from the lowest vegetable to those of the highest animal life, have, by the vicissitudes of heat and cold they necessarily undergo, a constant oscillation, a perpetual alternate contraction and expansion of all their solid and fluid particles.

35. So then the sun, though it be not the sole or chief fountain, is, as it were, the great regulator of motion, heat, and life to the inhabitants of this system; and on this account the benefits, to us men, of this great mover, this awakener and enlivener of nature, are highly to be prized; which, however, were much mistaken by the ancient heathen nations, and still continue to be so amongst many in the world, who having lost the true religion and philosophy of things, that were given to the first men, and so seeing things but by halves, and dazzled by the splendour of the sun's light and power, and sensible influences*, imagined, that for their life and all the goods thereof they depended

* Wisdom of Solomon, xiii. 1—7.

on that great luminary, and so worshipped the creature instead of the Creator, the work instead of him that made it, the knowledge of whom they had utterly lost. And this was never to be recovered without a new revelation, a new communication of himself from God to man, or a more accurate survey and knowledge of the system of nature than what mankind could of themselves soon or easily arrive at, by which the several connections, and subordinate dependences of its parts one with another, and the great and universal dependence of the whole on a wise and omnipotent Maker and Ruler might be found out.—An inquiry indeed of the greatest concern to us men, and in which there have been very great steps made in these latter times, that do very much confirm and illustrate the revelation given to Abraham for the conservation of the true religion, and continued to his posterity: which was afterwards much farther extended by the last grand revelation concerning the great Creator, the Lord of heaven and earth, that made the sun, earth, and stars, and governs them all with infinite wisdom and power.

IV. *Of the degrees of Heat in Animals.*

36. Thus much for the celestial and planetary heats. But now let us come nearer home, and

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consider

consider the various degrees of heat in the living inhabitants of this our earth, which we can see and feel, and examine by our senses: a very fruitful inquiry, and of greater immediate satisfaction and use to us, being what we are more particularly interested in.

The heat of animals is extremely various, both according to the variety of their kinds and the variety of the seasons. Zoologists have justly enough divided them into *hot* and *cold*, that is to say with respect to our senses. Those that are any thing near our own temperature we call *hot*, while all the others are reckoned *cold*, whose heat is much below ours, and consequently affects our feeling with the sense of cold, though, according to what trials I have had opportunity of making, they are all, by their *vis vitæ*, kept something warmer than the medium in which they live.

37. The gradations of life from the warmest animals down to perfectly inert matter are very slow, and by very small and almost insensible degrees, seeing, as it is not easy to determine the lowest state of vegetation, so too the limits between the highest vegetables and the lowest animals are with difficulty to be distinguished. Now all bodies partake of the heat of the medium surrounding them, as air, water, or any thing else. Nor can I find by the most careful experiments purposely made, that vegetables, any more than the most inert
and

and lifeless masses of matter, are endowed with any *vital heat* beyond the temperature of the ambient air, to be distinguished by the nicest Thermometers. As the air is a thin body, easily heated and cooled, so indeed when it is in a cooling state, as chiefly in the evenings and night season, it is colder than the plants in it. But on the other hand, for the same reason, through most part of the day it appears, and really becomes warm sooner and more sensibly than they can do. But, when all are kept in an equable state, there is no difference of heat to be observed, the plants always corresponding exactly to the temperature of the ambient medium. "In vegetabilibus & plantis nullus reperitur
"caloris gradus ad tactum, neque in lachrymis ip-
"sorum, neque in medullis recenter apertis *."

38. Nay the animals of low life have but very little additional heat beyond that of the air or water. The insect tribe greatly disappoint our expectations, seeming to be the most tender and delicate of all animals, and yet being those that can unhurt bear the greatest colds. They are preserved in the coldest seasons with little other shelter than the leaves and ragged barks of shrubs and trees, holes of the walls, or a very slight cover of earth; and some expose themselves quite naked and open. † Even in the bitter winters of 170 $\frac{8}{9}$ and 172 $\frac{8}{9}$ the

* Bacon Nov. Orig. II. 12.

† Boerh. Chem. I. p. 287, 415.

insect eggs, nymphas, and aurelias, outlived the colds that were intolerable to the livelier animals: and we know how vastly low the fluid then subsided in the Thermometers. Mr. de Reaumur* found some very young slender caterpillars that were able to bear a cold under *gr.* 4: and, what is still more, the French mathematicians in their Lapland journey in the autumn were pestered with innumerable swarms of flies of various kinds †, whose eggs and aurelias must have wintered under still greater colds than this: and I find that caterpillars have but a small degree of heat, about a division or two above the air they live in.

39. The whole insect tribe is commonly brought under the class of cold animals ‡: on which account I cannot but take particular notice of a very singular exception in the heat of bees, a very remarkable branch of that *genus* of living creatures. As these, according to the curious observations of naturalists, have something very peculiar in their economy, fabric, and generation, so I have ob-

* Mem. Ac. Sc. 1734, p. 257.

† Maupert. Fig. de la Terre, p. 12. See too Boyle Exp. of Cold, Abr. I. p. 661.

‡ Vid. Bacon Nov. Org. II. § 11. p. 167. § 12. p. 186. § 13. p. 192. Nat. Hist. 73. Lister de Bucc. p. 245.

served

served that they have a very singular constitution in the heat of their bodies. This I have experienced frequently; and I find that the heat of a swarm of bees raises a Thermometer buried amongst them above *gr.* 97; a degree of heat nothing inferior to our own.

40. The other animals of low life, agreeable to what I observed of the ordinary insects, have but very little additional heat above that of their supporting medium. I could scarcely distinguish any in muscles and oysters. It was very little in the branchial fishes. In flounders, whittings, codfish, and haddocks, the heat was found scarce a degree more than the salt water they were swimming in, even when that was so low as *gr.* 41. Nor are the red fishes much warmer than they. Some trouts, whose heat I examined were but at *gr.* 62, when the river water they had been swimming in was at *gr.* 61 *. Fish can live in water any thing warmer than freezing, that is, any thing above *gr.* 32.

41. The result of several trials made snails about 2 degrees warmer than the air. Frogs and

* And lately at Paris I found the heat of a carp scarcely to exceed *gr.* 54, the heat of the water wherein I examined it. The heat of an eel came out in the same way.

land tortoises seemed to have the principle of heat something stronger, *viz.* about 5 degrees higher than the air they breathed in. Which I suppose to be the case of the ordinary breathing animals, who indeed have lungs (but these lungs of a wide vesicular form) and have their blood not a great deal warmer than the branchial fishes. Such I take to be the sea tortoises, toads, vipers, and all the serpent kind, who all have their lungs of the same fabric, and the same cold constitution of blood. But most of this sort of animals are not able to bear very great colds; they in the severe seasons retiring into lurking places, where they are tolerably well sheltered; often perhaps at about the middle temperature of *gr.* 48 or so. Their *vis vita* is indeed in those seasons very low *, and their waste of substance exceedingly small †. And I suppose the same may be said of the swallows, and other sleeping birds and beasts, who, though naturally warm animals, much warmer than those already mentioned, are however in their inert state probably much colder than in their active and lively seasons.

* See Harv. de Mot. Cord. &c. IV. p. 28. XVII. p. 65. Leister. de Coch. p. 164. de Bucc. p. 246, 251.

† See Phil. Transf. Abr. II. p. 825. Leister de Coch. p. 163, 165.

42. For the heat of hot animals is not uniformly the same in all animals, and at all times. That admits of a very great latitude: it varies according to their various kinds and the circumstances of each. The surface of their bodies are, like other things, much affected by the heat or cold of the ambient medium, and consequently wrought upon by all the varieties of seasons and climates, if not sufficiently sheltered from their influences. When that is, their external and internal heat is nearly the same; but still differing something in different animals.

43. Dr. Boerhaave * indeed speaks of the heat of hot animals as in a natural state pretty uniform, or the same in all, reckoning it commonly to be such as to raise the mercury in the Thermometer to *gr.* 92, or at most 94. And so too Dr. Pitcairn's † heat of the human skin at his *gr.* 17 falls near our *gr.* 92. Mr. Amontons ‡, by several trials, found the heat communicated by the human body to his Thermometer to be at dig. $58\frac{2}{11}$, $58\frac{5}{11}$, $58\frac{6}{11}$, $58\frac{7}{11}$, $58\frac{9}{11}$. These by calculation I find to fall about Fahrenheit's *gr.* 91, 92, and 93. Sir Isaac

* Chem. I. p. 192, 207, 213, 414, 415, 526.

† El. Med. II. 1. § 26.

‡ Mem. Ac. Sc. 1703. p. 235, 243.

Newton's * *gr.* 12, which he makes equivalent to the external heat of the human body, and to the heat of a bird hatching eggs, comes to *gr.* $95\frac{1}{2}$ in ours. Fahrenheit himself † places his heat of the human body and blood at *gr.* 96. And at this mark Dr. Muffchenbroek ‡ says, the Thermometer stands, when it is immersed in the blood flowing from an animal; though in another § place he speaks of *gr.* 92 or 94, as a pretty high degree of heat for our blood.

44. I have very carefully made numberless observations on this subject of the heat of animals: and from them I must be allowed to say, that I find all these reckonings much too general, and in the main considerably too low. I suspect that, at least in many of them, there was not sufficient time allowed to warm thoroughly the bulbs of their Thermometers; or perhaps at the time of trial the folks hands had not the natural sheltered heat. The human kind is amongst the lowest of hot animals ||: and yet I by the heat of my skin every way well sheltered, at a medium in an infinite

* Phil. Trans. Abr. IV. 2. p. 1, 3.

† Ibid. p. 18, 52.

‡ Eph. Ultraj. 1728, p. 679.

§ Eff. de Phys. p. 502.

Cæsalp. Qu. Perip. V. 6. p. 132.

number of trials, raise the mercury in the Thermometer to about *gr.* 97 or 98. In some it goes a little higher, in others it keeps something lower. Urine new made, and that into a vessel of the same temperature with itself, is scarcely a degree hotter than the skin, as I have found by many repeated observations. And that heat of the urine we may reckon nearly equal to the heat of the neighbouring *viscera*. Dr. Hales * found the heat of his skin to be 54, and that of urine new made 58 degrees of his Thermometer, which coincide with our *gr.* 99 and 103, if the calcul that was made of the correspondence of his Thermometer with Fahrenheit's had been perfectly exact.

45. In the mean time, as I just now said, the human kind is among the lowest of hot animals. Ordinary quadrupeds, as dogs, cats, sheep, oxen, swine, &c. by the heat of their skin raise the Thermometer 4 or 5 divisions higher than we, as to *gr.* 100, 101, 202, and some to 103 or a little more,

46. And the breathing or cetaceous fishes are as hot as they, as Dr. Boerhaave † very justly reckoned; though he ascribes too small a heat to

* Veg. Stat. p. 59.

† Chem. I. p. 415.

them,

them, and to all other respiring animals, when he confined it within the narrow bounds of 92 or 93 degrees. Those who have had occasion to travel into the East Indies * tell us, that the blood of the Manati Fish, or Sea Cow, was sensibly warm to the touch. And Mr. Richer †, a curious enough observer of natural things, found the blood of the porpus as warm as the blood of land animals. I myself upon trial found the heat of the skin of that amphibious animal the sea calf to be near *gr.* 102. In the cavity of the *abdomen* it was about a division higher. In all this, as in many other respects, agreeing with our ordinary land quadrupeds; which, in the structure and form of their *viscera*, the breathing fishes do very much resemble.

47. The Lord Chancellor Bacon ‡ gives it as a current opinion that the bird kind are very warm. They are indeed the warmest of all animals, warmer still than quadrupeds by 3 or 4 degrees, as I have found by experiments on ducks, geese, hens, pigeons, partridges, swallows, &c. The bulb end of the Thermometer being lodged in their

* See Ent. Apolog. &c. p. 207. Le Comte Mem. de la Chine, II. p. 343.

† See Du Ham. Hist. Ac. Sc. p. m. 157. Mem. Ac. Sc. 1666—1668. VII. p. 325.

‡ Nov. Org. II. 13. p. 193.

groin, the mercury was raised to *gr.* 103, 104, 105, 106, 107; and in a hen hatching eggs I once found (for it is not always so high) the heat at *gr.* 108.

48. It was, I presume, without sufficient ground that the just now mentioned noble lord * reckoned the external flesh of animals, when in action, to be hotter than their blood, &c. Generally in my experiments both on birds and quadrupeds the *viscera* were found near a degree hotter than the skin; though sometimes I could perceive but little or no odds. And a Thermometer thrust into a fistulous ulcer running in amongst the muscles of the thigh in a man, shewed a heat very little greater than that of the sheltered skin

49. As the circulation is so very quick and free, the heat of the blood in the arteries and veins is pretty much the same, and but a small matter, it may be a degree or so, above the common heat of the bowels, or near 2 degrees warmer than the skin; a much smaller difference than what Sir Isaac Newton † and Dr. Hales ‡ assign; according to whose reckonings that would be no less than 10 or 11 degrees.

* Nov. Org. II. 13. p. 193.

† Phil. Transf. Abr. IV. 2. p. 1, 2.

‡ Veg. Stat. p. 58. Hæmast. p. 98.

V. *Of the preternatural Heats of Animals.*

50. These then are the heats of animals as in my experiments they generally appeared to me. But they are not to be held as universal. Beside the varieties in different individuals, the heat even in the same animal does not keep always the same. For ordinary indeed, and in health, the variations are not great; but by diseases and extraordinary accidents it may be much altered, and brought both above and below the natural standard. I cannot say that I have yet had opportunities of making observations enough to lay down any thing sufficient or satisfactory concerning the heat of animals in preternatural or morbid states. However I think I may venture to say, that it is not so vastly great in fevers as some are apt to conjecture.

51. The medical writers all take notice of the constant and burning heats in ardent fevers. And we are told * of the Emperor Constantius and of some others, that they were of such a dry hot temperament, as, when heated by a fever, in some measure to burn the hands of those that touched them. But this burning heat no body then knew how to measure, or to preserve to us any distinct intelligible notion of it. The determining and af-

* See Bacon. Nov. Org. II. 13. p. 193.

fixing the due bounds to such things being reserved to this present age. The ingenious Dr. Hales* supposes the heat of the blood in high fevers to be about *gr.* 85 in his Thermometer, which comes to *gr.* 136½ in ours; a degree of heat which, I believe, no animal ever arrives at, nay which I reckon no living creature is able to bear. Dr. Boerhaave† furniseth us with some curious observations of animals very soon destroyed in air of *gr.* 146, who all died in less time than what would have been necessary to bring their bodies up to the heat of *gr.* 136. A Thermometer put into one of their mouths a little after its death stood at *gr.* 110.

52. Nor, on the other hand, would I suppose the fever heat so low, or that low heat so dangerous, as the great Dr. Boerhaave‡ seems to do. He is afraid of its coagulating the *serum* of the blood, reckoning that terrible mischievous work might be effected by a degree of heat not much above *gr.* 100. Whence Dr. Hales§, and Dr. Arbuthnot¶, led thereto by Dr. Boerhaave's authority, affirm,

* Veg. Stat. p. 60.

† Chem. I. p. 275.

‡ Aph. 96, 689. Chem. I. p. 343. II. p. 352, 353, 378, 213, 357, 358.

§ Hæmast. p. 104, 105.

¶ Eff. on Air, p. 114, 211.

that the natural heat of the blood of a human creature approaches very near the degree of coagulation. But I have seen fevers, and these not excessively violent neither, wherein I was assured, by observing the heat of the skin, that the blood was 5 or 6 degrees above *gr.* 100, without apprehending any such danger, or experiencing any such fatal effects*. Such a heat, if neglected or wrong managed, may indeed, as Hippocrates† very justly observes, dissipate the more thin and watry parts, and so gradually thickening the whole mass of blood, have bad enough effects that way; if it do not bring on a putredinous thinness. But directly to coagulate the *serum* of blood, or the white of an egg, by several trials I found a heat requisite beyond any thing ever we can experience in a living animal. They continue thin and liquid till the Thermometer points at *gr.* 156 or so. Which is an arithmetical mean betwixt the ordinary heat of human blood at *gr.* 99 or 100, and that of boiling water at *gr.* 212; exceeding the one just as much as it falls short of the other.

* In the ague I had lately, during the height of the paroxysm, the heat of my skin was *gr.* 106; and so that of my blood *gr.* 107 or 108. And farther, what is very remarkable, in the beginning of the fit when I was all shivering, and under a great sense of cold, my skin was however 2 or 3 degrees warmer than in a natural healthy state.

† IV. De Morb. XXIII. 23, &c.

52. But no animal, I say, can bear a heat near this. Beside the bad effects it would have upon the fluids, I know our nerves are not able to endure such a scorching heat. If that be but a few degrees hotter than our blood it comes to be intolerable. Sir Isaac Newton* and Mr. Amontons† felt water too hot for their hands stirring in it easily to bear at *gr.* 108. I suspect indeed the full heat of the water had not been thoroughly communicated to their too bulky Thermometers. I find it becomes scalding hot to my hands and feet at *gr.* 112 or 114. And so I reckon will most people find, who have not their organs too callous. We should not however judge of the delicacy of the body by the sensation of the hands, which in many folks by labour and use are rendered preternaturally insensible. Mr. Amontons's valet could bear water of the heat of *gr.* 122. And I have seen folks who could handle things freely I could not well touch. I doubt not but the hotter animals can likewise bear a greater heat than we. And, on the other side, fishes and cold animals may be scalded to death with a heat less than that of our blood. A perch, one of the most lively of the branchial

* Phil. Trans. Abr. IV. 2. p. 2.

† Mem. Acad. Sc. 1703. p. 236, 244.

fishes, died in three minutes in water heated but to gr. 96*.

53. At first, men often gave names to things from certain resemblances, without pretending thereby to understand or exhibit their natures. And in this view physicians may continue to talk of the *το πυρ*, the *febris ardens*, and *inflammations*, and freely use these and other fiery names, if they mean no more by them (as it is likely they did not in the original use of them) than a figurative way of expressing extraordinary heats of the body, or the heat of the blood carried considerably beyond its natural state. But from undeniable experiments we see they must be much mistaken, who not confining themselves to the figurative use of these names, and led astray by vain and ill-grounded theories, talk of real scorplings of the body, and actual burnings of the heart and other *viscera*, of ebullitions and effervescences of the blood, of its being kindled and ready to burst into flame †, and of other such fiery descriptions of heats, which no animal, while in life, can be brought into. So that these are ways of speaking, which, to prevent mistakes, should have no place out of poetry; where alone such bold figures, and these expressed in such a strong manner, without any restriction,

* Muffchen. Tent. Exp. Ac. Cim. Add. p. 122.

† Willis Pharm. Rat. II. p. 22.

are to be allowed. Thus some people by their profession may be permitted

*Jecur to burn, and Cor to pierce,
As either best supplies their verse*.*

But for others, who from the nature and great consequence of the subject they deal in should be more strictly tied to truth, gravely to talk of the liver burning up the bile, and speaking of the heart as they would of a piece of red-hot iron, burning the hands of those that touch it †, or boiling up into froth the blood that falls into it ‡; and others to give out the stomach as a kettle boiling our food by its great heat §; such assertions, I say, as these are unwarrantable impositions on the senses of mankind, and should be dropt by all sober thinking men.

* Prior, Alma, p. 87.

† Abu. Jaafar Ebn. Tophail. Philos. Autodid. p. 64. Ali Rodoan apud Gom. Pereir. Antonian. Marg. p. 326, 770. Columb. de Re Anat. XIV. p. m. 477, 481. Diemerbr. Anat. II. 6.

‡ Des Cartes de Meth. 5. p. 29. De Hom. p. 8. De Form. Fœt. § 18, 72. De Pass. I. 8, 9. Epist. I. 52.

§ Hipp. apud Cels. Med. Præf. p. 6. Stukely of the Spleen, p. 35.

54. We see then that nature seldom carries the body or blood of animals to a degree of heat very much beyond its ordinary healthy temperature. Nor indeed are we able to bear any very great excess of that kind, either in the body itself, or in the medium without us. We can however, on the other hand, endure greater excesses of cold, at least externally affecting us; there being many remarkable instances of the surface and extremities undergoing very great colds without being absolutely destroyed. We have all, I suppose, some time or other, even here, experienced very sharp nipping colds. These, however, are nothing, compared to the frightful accounts we have had of the great prevalency of cold on the bodies of men and other animals in the more northern countries. Often are they quite frozen to death; but, at other times, though the cold has gone a great length, they are happily recovered. We have been told* of animals found stiff cold in their lurking places, or buried in lumps of ice, that, by the *stimulus* of pain or a warmer air, have been again brought to their life and senses. Partial freezings of the nose, hands, feet, &c. of the human body are frequent. But, what is much more, there are instances † of

* Boyle Exp. on Cold, Abr. I. p. 671, 673. Quésnay Oecon. Anim. p. 25.

† Fabr. Hild. de Gang. &c. XIII. p. 792.

men being so frozen by the lump, that when plunged into cold water they contracted a cover of ice over their whole body, which therefore in all its exterior parts must have had a cold considerably under *gr.* 32. If it came so low as *gr.* 25 the blood must have been frozen in some of its vessels. However, even in this terrible threatening state, when the whole body seemed to run such a risque of congelation and the sleep of death, by good management, well understood in these intemperate countries, people have been recovered to their life again. In such extreme cases one would think that the internal heat too must have been considerably impaired; but how low that may be carried without danger or immediate death we cannot yet determine. In the mean time, the measuring the heat of the birds and beasts that pass the winter in a state of sleep and insensibility, might give us some light into this matter.

Thus much in general of the heat of animals: that subject having been more minutely handled in another treatise, *De calore animalium*.

What follows is more in the nature of a plan of what is proposed or should be done on the respective heads, than any thing fully executed on them.

Heads to be added.

VI. Of the heats of waters, oils, and salts, according to their fluidity and consistency.

VII. Of

VII. Of the melting and shining heats of metals and minerals.

VIII. Of the boiling heats of liquid and melted bodies.

IX. Of the heats of flame and fire.

VI. *Of the Heats of Waters, Oils, and Salts, according to their fluidity and consistency.*

The consistency or fluidity of most bodies are no essential or permanent properties in them; but are rather a sort of circumstances or accidents manifestly depending on the various degrees of heat applied to them. Some of the hardest bodies may be reduced to a fluor by fire. Stones, metals, salts, &c. some by less, some by greater degrees of heat may be melted down into fluids; which only continue so as long as the requisite heat lasts. On the other hand, most of those bodies which to us commonly appear in a fluid state, by cold, or the diminution of heat, may be brought to a consistent form. And if there be some fluids which have never been seen to freeze, this may, with a great shew of reason, be ascribed to the difficulty, or perhaps impossibility, of getting altogether rid of heat. And for the greater illustration of the doctrine of heat, and to get a farther view into the

the natural history and philosophy of bodies, we will here endeavour to settle the limits of firmness and fluidity in a great many of them, as far as our own or other people's observations can carry us: for the greater uniformity reducing them all to Fahrenheit's scale; which we will suppose protracted even much beyond the heats his Thermometer could bear.

It is a bold but a very just expression of Sir Isaac Newton*, when he calls "*Water* a fluid tasteless salt; which," adds he, "nature changes by heat into vapour, and by cold into ice, which is a hard fusible brittle stone, and this stone returns into water by heat." Agreeable to which view of things, Dr. Boerhaave† calls water a kind of glass that melts at a heat any thing greater than *gr.* 32; the boundary between water and ice.

All our ordinary liquors are but water with an additional mixture of oils, salts, and earth; and have all or most of them been observed to freeze, some at a greater and some at a less degree of cold under the freezing of common elementary water.

The animal liquors generally lose their fluidity easily enough. Dr. Boerhaave‡ speaks as if blood

* Opt. p. 349.

† Chem. I. p. 399, 614.

‡ Chem. II. p. 377.

would

would freeze as soon as water, viz. at *gr.* 32. But on trial it is found to resist the cold something more. The blood of a lamb was not frozen at *gr.* 29; but it was turned into ice at *gr.* 25*. And some of the cold animals preserve the fluidity of their liquors still more obstinately. Some insects indeed were killed, and their bodies hardened, at about *gr.* 20†. But there are others that resisted the cold below *gr.* 4; preserving the life and softness of their bodies, and the fluidity of their juices‡.

Milk is not ill to harden, freezing I think at about *gr.* 30.

Ordinary *urine* freezes at *gr.* 28, though if it be strongly impregnated with salts and oils it may continue fluid in a cold somewhat greater.

Fermented liquors do not freeze so easily as simple water; and the stronger they are they can bear the greater cold; and when they are frozen, it is the weakest parts that suffer first, the stronger and more spirituous often continuing fluid. Small beer and small wines easily turn into ice; and the great colds in Muscovy, Greenland, Hudson's Bay, &c.

* Reaumur in Mem. Ac. Sc. 1734. p. 260.

† Ibid. p. 259.

‡ Ibid. p. 257, 258.

froze the strongest wines, as strong claret, Malaga, Alicant, &c. *

Such colds were certainly very great; but those who had occasion to observe them, had not the means of taking any exact estimate or determined measure of them; which, if an opportunity were offering, we can do now with greater certainty †. Not only the strongest wines may be frozen. Brandy itself has been converted into ice ‥; the stronger part, if there be bulk enough of the liquor, usually retiring to the center, (as in the freezing of strong wine), and there constituting a stronger spirit of wine separate from the rest. And such must undoubtedly have been a very great cold. Brandy eluded Dr. Musschenbroek's ‡ attempt to freeze it with the cold of snow and beaten ice. A mixture of tolerably well rectified spirit of wine and water in equal parts, which I reckon would make a liquor about the strength of common brandy, was not frozen but with the cold of —*gr.* 7. A weaker mixture of 1 part spirit to 3 of water froze at *gr.* 7. But, on the other side, a

* See Boyle Exp. on Cold, Abr. I. p. 594, 607, 609, 610. Scept. Chym. Abr. III. p. 281. Phil. Transf. Abr. II. p. 152. Merret Exp. of Cold, p. 47, 48.

† Last winter I found good Burgundy, strong claret, and Madeira, to freeze at about *gr.* 20.

‥ Boyle Exp. of Cold, Abr. I. p. 594.

‡ Tent. Exp. Ac. Cim. Add. p. 176.

strong

strong mixture, where the spirit was double the water, did not freeze till the cold came down to —gr. 11*.

But Mr. de Reaumur's rectified spirit of wine itself underwent in the winter at Torneao a cold greater than any thing he was able to make in France. There too it was frozen†, though at what degree we cannot tell. Only we are sure that the cold able to do it was no greater than —gr. 34; perhaps a good deal less. If people were apprized and on their guard, this might be accurately enough determined in such cold countries; where, I suppose, the most perfect alcohol might be frozen, and perhaps even the lately invented ethereal spirit of wine itself.

Vinegar is the produce of a second process of fermentation, whereby all the oil and spirits of the wine are converted into a lean acid liquor, that resists cold pretty much, and (like wine and ardent spirits) the stronger it is, the more. Common vinegar is indeed not difficult to freeze, that may ordinarily happen at about gr. 28. But when freed of a great deal of its phlegm by distillation, under the name of spirit of vinegar, it resists a much greater cold. However, it was frozen when exposed to an intense cold, as Mr. Boyle‡ tells us;

* Reaumur in Mem. Ac. Sc. 1734, p. 255.

† Mupert Fig de la Terre, p. 38.

‡ Exp. on Cold, Abr. I. p. 593.

as also when surrounded by an artificial mixture of snow and spirit of nitre. But Dr. Musschenbroek*, who tried the experiment after Mr. Boyle†, neglected to observe, or at least to tell us, the cold of that vinegar ice. In this freezing there was a part of it continued fluid; the strongest and most concentrated vinegar that can well be imagined. And yet this too, by a new application of the cold mixture, was wholly turned into ice‡; whereby its strength and acidity was considerably impaired.

But the strong *mineral acid spirits* preserve their fluidity much better; excepting however oil of vitriol, which begins congelation (or coagulation rather) near as soon as fair water||. In the trials which Mr. Boyle† and Dr. Merret§ made in the very cold weather, they found nothing of freezing in aqua fortis, spirit of nitre, spirit of salt, vitriol, &c. Nay, the ordinary cooling mixtures do not affect them. But Fahrenheit, by

* Tent. Exp. Ac. Cim. Add. p. 175.

† Exp. on Cold, Abr. I. p. 590.

‡ Musschenb. ibid. p. 176.

|| Merret Acc. of Freez. p. 8. Confer. & Boyle, Abr. I. p. 396, 559. III. p. 482. But it seems this its disposition to freeze or coagulate goes off by time. See Musschenbr. Eff. Phys. § 920.

† Boyle Exp. of Cold, Abr. I. p. 596.

§ Merret Acc. of Freez. p. 17.

reiterating such cold affusions produced a very intense cold at —gr. 40, whereby spirit of nitre itself congealed and shot into crystals *.

The alkaline spirits do not seem to be endowed with so great a power of resisting cold. Spirit of urine and spirit of human blood were frozen in cold weather in England †. And a cooling mixture of snow and spirit of nitre congealed spirit of sal ammoniac made with lime ‡. This mixture was able to freeze spirit of vinegar, and weak spirit of salt, and to turn other saline liquors into figured ice. But it had no discernible influence on spirit of nitre.

Water impregnated with a moderate quantity of any of the ordinary salts freezes in cold weather; as with sugar, alum, vitriol, nitre, sea-salt, verdigrease, arsenic, sal ammoniac, kelp, lixivate salt, &c. § But a solution of sal gem did not freeze with the rest ||. And oil of tartar *per deliquium* was brought to congeal only by a

* See Boerh. Chem. I. p. 164, 165.

† Boyle Exp. on Cold. Abr. I. p. 593, 597.
Exp on Blood, III. p. 482.

‡ Ibid. p. 590.

§ Boyle Exp. on Cold, Abr. I. p. 590, 593, 596.
Merret Ibid. p. 4, 5, 7.

|| Merret Ibid. p. 7, 8, 47.

mixture of snow and salt *. Sea water freezes, whatever has been inadvertently said to the contrary. The northern voyages are full of this †. The Zuyder Sea in Holland is often frozen up. And the Baltic is a strong instance offering itself every year of the sea water being converted into ice. Nay it freezed at London with Mr. Boyle ‡. And I have found small parcels of saline bitter ice in some places of our sea rocks, where the water had been tossed up, and allowed to stagnate. The freezing point of sea water is at *gr.*

But water, when fully saturated with those salts, doth not easily freeze §. That such solutions resisted the colds in England will not be wondered at. But we are told that a strong brine of bay salt cannot be frozen even in the northern countries ||. And syrups, which are but strong solutions of sugar and water boiled up together, are not congealed with any ordinary cold **.

* Boyle Ibid. p. 593, 596.

† See Musschenbr. Ess. Phys. § 925.

‡ Exp. of Cold; p. m. 147.

§ Boyle Exp. on Cold, Abr. I. p. 597. Merret ib. p. 6.

|| Phil. Trans. Abr. II. p. 152.

** Merret. Ibid. p. 52, 53.

Of Oils.

Oils too may be frozen, or at least coagulated by cold. And some of them, as the resins, common sulphur, spermaceti, tallow, butter, &c. exist ordinarily in a consistent form, though in their nature they are as truly oils as those that most obstinately retain their fluid state. But as oils are gradually thinned by heat or thickened by cold, the limits between their consistency and fluidity are not so exactly determinable in them as in watry substances.

Very many oils preserve their fluidity in great colds *. We have not heard of the ordinary distilled oils being frozen; except that once Mr. Boyle† found that unrectified oil of turpentine had its upper part turned to ice. And oil of anise for ordinary appears in a sort of butter state till it be warmed to *gr.*

Even many of the expressed oils stand out the cold very well; such as oils of lintseed, hempseed, nuts, sweet almonds, &c. ‡. But common

* See Boyle Exp. on Cold, Abr. I. p. 596, 597. Orig. of Heat, &c. Abr. I. p. 559.

† Ibid. p. 597.

‡ Boyle Orig. of Heat, &c. Abr. p. 559. Exp. on Cold, Abr. I. p. 598.

oil of olives stiffens and grows opaque very soon, as at about *gr.* 43. But different oils, and different parts of the same oil freeze at different degrees of heat. So that we see some part of this oil sometimes continue fluid in pretty strong frosts.

Wax is a vegetable oil collected from flowers by the bees. This swims on water, and does not lose its firmness and opaqueness till that be heated to *gr.* 142.

All the resins may be looked on too as vegetable oils, and they melt with a heat below that of warm water. Common rosin comes to a sort of fluid by the heat of about *gr.*

Many of the mineral oils appear for ordinary fluid, as petroleum, naphtha, &c. nor are we yet informed at what cold they become firm. Others again, as asphaltos, succinum, &c. we find always in a firm dry form; but which may be melted by heat. And sulphur too is such an oil. And flowers of brimstone, that is to say, pure sulphur, melts into a fluid with the heat of *gr.*

Train oil does not easily freeze*. And I suppose it was from this that Olaus Magnus† tells us, that it is usual in the northern regions to fill up the frozen ditches of fortified places with train oil in the winter, to preserve the lower water fluid.

* Boyle Exp. on Cold, Abr. I. p. 596, 598.

† Vid. Ibid. p. 596.

But Mr. Boyle * and Dr. Merret † found it frozen even by the cold weather of England.

But the other ordinary animal oils rather take a considerable heat to keep them fluid. Butter is such an oil. And that in the month of May took a heat between *gr.* 80 and 90 to oil it : but being so heated it keeps something thin till it fall to about *gr.* 74.

Hogs lard.

Tallow.

Spermaceti is the firmer part of the oil of the Macrocephalus or Dentatus whale ; and does not melt with a heat under *gr.*

Of Salts.

We lately proposed common water as a tasteless salt dissolvable by a small degree of heat. What if, on the other hand, we should reckon the ordinary salts as different waters brought to a liquid state by a greater degree of fire, some sooner, some later.

Alum melts into a fluid at *gr.*

Sea salt.

Borax.

Nitre.

Sal ammoniac.

Strong alcali salt.

Vitriol.

* Boyle Exp. on Cold, p. 596.

† Merr. Ibid. p. 47.

VII. *Of the melting and shining Heats of Metals and Minerals.*

And now are we come to consider those, which of all terrestrial bodies, except elementary earth itself, are the most consistent and firm, and most susceptible of the greatest heats; I mean metals and mineral substances. They are naturally in a consistent form: but some of them flow at a moderate enough fire; while others require so intense a heat to dissolve them, that we find it difficult how to bring it to a calcul.



Quicksilver is always reckoned in the class of metals. But it is so far from the common consistent form and appearance of other metals, that it may justly be looked on as the truest, visible, and tangible fluid in the world. By some slight additions it may indeed be robbed of that property. But when left to itself pure and unmixed, it always preserves its fluidity in the very greatest colds natural or artificial that have ever been observed*.

* See Boyle Exp. on Cold, Abr. I. p. 597. Boerh. Chem. I. p. 36, 165. De Maupert. Fig. de la Terre, p. 58.

¶ h §

Of all the ordinary simple metals or metallic substances *tin* loses its consistency, and flows the easiest, or with the lowest heat. By Sir Isaac Newton's observation *, reduced to the numbers of our Thermometer, it melts at *gr.* $(\frac{71 \times 180}{34} + 32 =) 408$. Dr. Musschenbroek's experiments † carried it but a little higher, *viz.* to *gr.* $(\frac{109 \times 180}{53} + 32 =) 422$. And there may be greater differences from accidental mixtures. Sir Isaac's experiments of most of the heats below this of tin are, I believe, pretty near the truth. The heats beyond this being deduced in a more precarious way, we cannot be so sure of.

Lead is the metal next to tin that fluxes easiest. But here I do not find different folks observations agreeing so well as in the other. In Sir Isaac's way of finding out the heat of lead beginning to melt, that would fall on our *gr.* 540 ‡. But according to Musschenbroek's experiment § reduced to our numbers, the mercury by such a heat should rise to *gr.* 769; were it susceptible of such a great

* Phil. Trans. Abr. IV. 2. p. 2, 4.

† Tent. Exp. Ac. Cim. Add. II. p. 21.

‡ Ibid. p. 2.

§ Ibid.

expansion in a regular manner. And Mr. Amon-
tons* found gun-powder to be kindled by the same
heat that melted lead.

Some bodies, by being combined into one mass,
become more firm than either were in a separate
state. And, on the other hand, others, when put
together, make a compound more dissolvable than
its ingredients; much contrary to our expectations.
So little should we pretend *a priori* and without ex-
periments to determine the nature and properties
of bodies.

Thus lead and tin may be mixed together in
such proportions, as that the whole shall melt more
easily than even tin itself †. A compound of three
parts tin and two of lead Sir Isaac found to melt
at his *gr.* 57, or our *gr.* $\left(\frac{57 \times 180}{34} + 32 =\right) 334$ ‡.
But if the lead be in a greater proportion to the
tin, a higher heat than that of melting tin is requi-
site to make the compound flow. For he expe-
rienced a heat of *gr.* 460 to be required to melt a
mixture of four parts lead and one part tin §. When
the lead was to the tin as 5 to 1, that heat was
not able to keep the mixture in fusion ¶.

* Mem. Ac. Sc. 1703. p. 247.

† Muffchenb. Ibid. Boerh. Chem. I. p. 751.

‡ Phil. Transf. Abr. IV. 2. p. 2.

§ Ibid.

¶ Ibid.

What still vastly increases the fusibleness of these metals is the addition of bismuth or tin-glass, a marcasite or sulphureo-metallic substance, not so easily melted as tin. That bismuth Sir Isaac Newton used in these curious experiments, we shall just now have occasion to copy from him, did not melt with a heat under *gr.* 460 *. And Dr. Musschenbroek's bismuth was much harder to dissolve, to wit at *gr.* 1051 †. And a golden marcasite, though more fluxil, took *gr.* 506 to melt it.

A mixture of tin and tin-glass melts at a heat less than what is requisite to melt either separately ‡. When they were in equal quantities the compound melted at *gr.* 283 ||. If you alter this proportion the mixture becomes susceptible of a greater heat. When the tin was double the bismuth it did not melt with a heat under *gr.* 334 §. And it stiffened in this heat when the tin was to the bismuth as 5 to 3 **. And if the proportion of bismuth be very small, the heat required to keep the compound in fusion is still greater, and comes nearer to that of melting tin. When there was eight times more

* Phil. Transf. Abr. IV. 2. p. 2.

† Musschenbr. Ibid.

‡ Ibid.

|| Newton, Ibid. p. 2.

§ Ibid.

** Ibid.

tin than bismuth in the mixture it would not melt with a heat under *gr.* 392 *.

We are told by Sir Isaac that the heat of melting lead was *gr.* 540, and that of bismuth 460. But these two bodies, so difficult to be melted by themselves, when blended together into one mass, are much easier brought into fusion. A mixture of them in equal quantities but just stiffened at *gr.* 334 †; and so would melt with very little more heat.

But what is most surprising of all is the great fusibleness of a mixture of all these three ingredients. Mr. Homberg ‡ proposed for an anatomical injection a compound metal of lead, tin, and bismuth in equal parts, which he tells us keeps in fusion with a heat so moderate that it would not singe paper. Sir Isaac contrived a mixture of them in such proportions that it melted and kept fluid with a heat still smaller, not very much exceeding that of boiling water. A compound of two parts lead, three parts tin, and five of bismuth but just stiffened at that very heat §; and so would have fluxed with very little more. And another mixture of the same materials, in a proportion somewhat different from that, actually melted by the heat

* Newton, Ibid. p. 2.

† Idem, Ibid.

‡ Mem. Ac. Sc. 1699. p. 235.

§ Newton, Ibid.

of *gr.* 246 *. It consisted of the lead, tin, and bismuth, in the proportions to one another of 1, 4, and 5.

Antimony is a mineral compound of sulphureous and metallic parts. The metallic substance when separated from the other is called its *Regulus*. This is able to bear a great heat without melting. When prepared with iron, and so going under the name of *Regulus Martis*, it was found to stiffen only at *gr.* 805 †, and so would take a heat something greater thoroughly to melt it.

But this regulus fluxes something easier when mixed with tin or bismuth. A mixture of it and tin in equal portions was found to melt at *gr.* 635 ‡. When the regulus exceeded five times the quantity of tin it stiffened at *gr.* 752 ||. And so too did a mixture of regulus and bismuth in the proportions of two to one §. But when the regulus was in lower proportion to the bismuth, as in the ratio of four to seven, the compound was easier melted; for it stiffened at *gr.* 635 †.

Though it would seem by these experiments that

* Newton, Ibid.

† Idem, Ibid. p. 3.

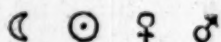
‡ Idem, Ibid.

|| Idem, Ibid.

§ Idem, Ibid.

† Idem, Ibid.

the metal of antimony gave the tin and tin-glass a power of resisting the fire, and that even something greater than its proportional quantity in the mixtures should require; yet it promotes the fluxibility of the harder metals. When added to red-hot iron it soon brings it into fusion*. And it increases the fusibleness and volatility of some of the other metals†.



The *hard metals*, silver, gold, copper, and iron, require a more intense degree of heat to destroy their consistency, and bring them into a fluid state. And before that too they undergo a very remarkable change, appearing in a shining form; when we know that they have a vast burning force, far exceeding the heat of ordinary hot bodies, as the summer air, boiling water, &c. the proportions of which some have endeavoured to determine.

Dr. Keill‡ affirms “the heat of red-hot iron to be only seven times greater than the ordinary heat of the sun in summer.” But this ordinary heat of

* Boerh. Chem. I. p. 55. II. p. 511.

† Ibid. I. p. 34, 38.

‡ Exam. of Burn. Theor. p. 156.

the sun in summer is not a very definite thing, especially as then understood. So that we are not quite sure what Dr. Keill really reckoned to be the heat of red-hot iron. Though I suppose he had some view to what he found in Sir Isaac Newton on that subject.

Very early had this great man been making experiments on the various degrees of heat in bodies. And it was from these his first trials he had what he says of them in his *Principia*, reckoning the heat of red-hot iron to be three or four times greater than that of boiling water *: almost as Dr. Pitcairn soon after gave out the matter in his colleges at Leyden †, to whom I am ready to think Sir Isaac had communicated this observation, that “by the heat of our summer air the oil is raised in the Thermometer $7\frac{1}{2}$ degrees; by the heat of our skin to gr. 17; by boiling water to gr. 50 or 52; by the heat of red-hot iron to thrice that number,” or to gr. $(180 \times 3 + 32) = 572$ in our scale.

But upon what grounds Sir Isaac reckoned red hot iron (*ferrum candens*) to be but three or four times hotter than boiling water I cannot well tell. Nor how either he or Dr. Pitcairn could determine the dilatation of linseed oil by the direct application of the heat of red-hot iron, can I conceive any way possible; for it boils by a much less heat than

* Princip. p. 508.

† Elem. Med. II. 1. § 26.

that. And, if you were to imagine oil to expand regularly as far as you please without boiling, the heat of red-hot iron would carry it much beyond 150 or 200 such degrees; that is to say, it would dilate the oil much more than three or four times the expansion given it by boiling water.

It is true Sir Isaac gives out this computation only as a conjecture, which however, as it stands in the first edition, is still preserved without the least alteration in both the other two: though he had, long before he published them, made some new experiments, whereby in a very ingenious way he had found the heat of red-hot iron to be a good deal higher: for his first inquiries concerning the comparative heat of bodies must (as is commonly the fate of all first essays) have laboured under some imperfection. But, as this affair was well worth prosecuting, his great and inquisitive genius did not rest in them. He repeated his trials with greater care and exactness, and *an.* 1701 gave in to the Royal Society the result of them in that excellent paper, (to which already we have been so much beholden for so many curious experiments and useful hints), containing *a scale of the degrees of heat* determined with an oil Thermometer and a lump of red-hot iron*.

* Phil. Trans. Abr. IV. p. 1, &c.

Of this method of determining the degrees of heat we have already* had occasion to speak more fully. It is the result of his observations we are just now concerned with. And we are told †, that at his *gr.* 114, which coincides with our *gr.* $\left(\frac{114 \times 180}{34} + 32 =\right)$ 635 heated bodies begin to shine in the dark, but just so as to be perceptible. This may be called the *shining point of heat*. But I would suppose, from the train of Sir Isaac's experiments, that this and what he says farther of the splendour of heated bodies is to be meant solely of iron: for it is likely enough, that different metals and other bodies may arrive at their shining heats in different degrees.

But iron must be vastly heated beyond this to have the character of being red-hot. Before it can shine briskly even in the dark, it must be at *gr.* 752 ‡: and yet then it has no perceptible splendour in the twilight. And it is not till the heat arrive at *gr.* 884, that bodies shine distinctly in the twilight immediately before sun-rising or after its setting ||, but not at all or very obscurely in strong day-light: and, agreeable hereto, we find among

* Ess. on the Heating and Cooling of Bodies, § 3.
 &c.

† Phil. Trans. *ibid.* p. 3.

‡ *Ibid.*

|| *Ibid.*

Dr. Musschenbroek's experiments * an iron rod heated, but not reddened in the fire to have been lengthened 276 deg. of his Pyrometer; which falls on *gr.* $(\frac{276 \times 180}{53} + 32 =) 969$ in our scale.

In Sir Isaac's reckoning the heat of a small pit-coal fire unblown, and of iron heated red-hot in it, was *gr.* $(\frac{192 \times 180}{34} + 32 =) 1049 \dagger$: and a rod of steel heated in the fire till it was red, Dr. Musschenbroek ‡ found to be lengthened 364 of his degrees, and consequently to have been heated to our *gr.* $(\frac{364 \times 180}{50} + 32 =) 1095$, you see not differing much from Sir Isaac's heat of a small pit-coal fire.

But he || gives out the heat of a small wood-fire as greater, amounting perhaps to his *gr.* 200 or 210, which is our *gr.* $(\frac{210 \times 180}{34} + 32 =) 1408$: and a greater fire he justly reckons hotter still, especially if blown with bellows. Copper shining hot from the fire Musschenbroek ** lengthened 392 divisions, that is, to our *gr.* $(\frac{392 \times 180}{59} + 32 =) 1228$.

* Tent. Exp. Ac. Cim. Add. II. p. 47, 48.

† Phil. Transf. *ibid.*

‡ Tent. &c. *ibid.* p. 48, 49.

|| Phil. Transf. *ibid.*

** Tent. &c. *ibid.* p. 49, 50.

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And, to be sure, iron, in reddening and melting, takes on a greater heat than copper.

This matter to be farther prosecuted by experiments.

VIII. *Of the boiling Heats of liquid and melted Bodies.*

A very moderate degree of heat, no greater than that of hot animals, is sufficient to generate air bubbles in fresh *water*. But, to make it really boil, a vastly greater heat is required. By the very construction of our Thermometers that happens when the mercury is raised to *gr.* 212; beyond which, water is incapable of being heated in an ordinary state of the atmosphere: which circumstance is to be understood too of the *boiling heats* of the other fluids we shall have occasion to mention.

Sea water in boiling does not take on a heat sensibly greater than common water: but, if the water be as strongly impregnated with salt as it is capable of being, it is susceptible of some more heat. Dr. Boerhaave* found such a strong pickle boil up to the heat of *gr.* 218.

A *lixive of pot-ashes* is heated to *gr.* 240 before it boils†: and the common *saline spirits*, which

* Chem. I. p. 746.

† Phil. Transl. Abr. VI. 2. p. 18.

are strong solutions of volatilized acid salts in water, bear this great or a greater degree of heat. Spirit of nitre comes to *gr.* 242 * : and I suppose aqua fortis, spirit of salt, &c. are susceptible of much the same. But oil of vitriol, that strongest of all the acid spirits, (and oil of sulphur *per campanam* is much akin to it), goes to a vastly greater heat, *viz.* to *gr.* 546 †.

But the common true *oils* require still a much greater heat to bring them to a boiling. It is true *alcohol* boils with the lowest degree of heat of any liquor, to wit at *gr.* 174 or 175 ‡, and brandy at about *gr.* 190, as I have experienced : and alcohol is but a thin oil ; but then, by the processes of fermentation and distillation, it is so much subtilized and attenuated, as to become thinner, more volatile, and easier boiled than water itself, as Lord Verulam too, long ago, observed ||.

Those oils that have any lentor in them have a much stronger cohesion of parts, and can bear a much greater heat. “ *Majorem calorem desiderat ad hoc ut bulliat oleum, quam aqua ; & tardius multo bullire incipit,*” says the same Lord

* Phil. Trans. Abr. VI. 2. p. 18.

† Ibid.

‡ Ibid. and Boerh. Chem. I. p. 168, 745, 746, 749.

|| Hist. Dens. &c. p. 46.

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Verulam *. Oil of turpentine is amongst the thinnest of all these, and yet in boiling it raised the mercury in the Thermometer almost to *gr.* 560 †: and hereabout I reckon the boiling heat of the other ordinary volatile or stillatitious oils; which however, as the boiling continues, is always augmenting, the more volatile parts flying away, and leaving the residue thicker, stronger, and susceptible of greater heat ‡.

The *expressed fixed oils* can take on still a greater heat. I find Mr. Fahrenheit and Dr. Boerhaave speaking as if they and quicksilver began to boil at much the same degree of heat, *viz.* about *gr.* 600 ||: and in boiling they are susceptible of still a much greater heat than this before they burst out into flame. Dr. Musschenbroek has furnished us with a method of judging of that heat, independently of the common Thermometers, from the lengthening of metals expanded in it, by which the heat of boiling oil came out considerably higher than what is assigned to mercury boiling. From the freezing point he found ** the elongation of

* Hist. Denf. &c. p. 46.

† Boerh. Chem. I. p. 747.

‡ Ibid. and p. 748. and Phil. Transf. ibid.

|| Fahr. in Phil. Transf. ibid. p. 18, 52. Boerh. ibid. p. 165, 265, 291, 747, 748, 752. Musschenbr. Eff. Phys. § 880, 952, 896.

** Tent. Exp. Ac. Cim. Add: II. p. 20.

an iron rod in rapeseed oil so boiling as to be ready to burst into flame to its elongation in boiling water, as 201 to 53, and consequently as 682 to 180: so that, if the mercurial Thermometer had been capable of being lengthened out far enough, this boiling oil would have raised the mercury in it to $gr. 682 + 32 = 714$; and that is a heat in the ordinary way of reckoning from above the freezing point, ($\frac{682}{180} = 3\frac{4}{5}$), near four times greater than that of boiling water.

It is true, a much lower heat made a sort of boiling noise and froth in the oil, arising from the expulsion of its more watry parts. This came on when the iron rod was lengthened but 120 parts, and that lights about $gr. 440$ in our Thermometer. But this is but an accidental and indefinite sort of heat, and not at all to be reckoned the complete boiling state of the oil, or the greatest heat it is capable of without burning.

F I N I S.



